

15.2.1

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fluorescence begins to decline. Within about 10 seconds, a steady state in the processing system is established, and there is a relatively low, steady level of fluorescence (Figure 2).

The pattern of fluorescence change to this point is called the fluorescence induction curve. Over longer periods (minutes), there may be another, slower increase in fluorescence and a new plateau in fluorescence reached as processes further downstream from the initial events become saturated and begin to regulate the upstream flow.

Fluorescence induction is a sensitive probe for photosynthesis over very short periods, and responds to any changes affecting photosystem II activity. Thus, it has been very important in elucidation of photosynthetic mechanisms (Papageorgiou, 1975). More recently, it has been used in studies investigating the effects of various types of stress on plants. Many types of stress - water, heat, excess light, salt, chilling, freezing - result in changes in fluorescence induction characteristics long before there are changes in other measureable characteristics (Smilie & Hetherington, 1983). There appear to be no studies on stress to foliage that have indicated effects on other processes without there being effects on fluorescence induction.

Changes in fluorescence curves brought about by stress can be interpreted to provide presumptive evidence about the way in which the stress is affecting photosynthesis. For example, a reduced peak in fluorescence could indicate damage to the light receptor system; increased peak fluorescence could be due to damage on the carrier side of PS II close to the reaction centre, and a slow decline from the peak could be due to damage further downstream.

To our knowledge, this technique has not been previously used in studies of oil and/or dispersant damage to plants. We had several objectives in applying it in this study:

- i) We wished to determine if any of the treatments affected the photosynthetic system, and how such effects varied between treatments and zones.
- ii) By comparing the fluorescence induction parameters with other criteria of stress (growth rates, flowering, etc.) measured at different times, we wished to determine if this method could be used to forecast stress effects that would otherwise be apparent only after longer intervals.

iii) Assuming that changes in the chlorophyll induction curve would be one of the first responses to stress, we wished to use the method to indicate how quickly after applying oil and/or dispersant, the plants begin to be stressed.

APPENDIX 2: SUMMARY TABLE OF ANALYSIS OF VARIANCE RESULTS FOR THE THICKNESS
OF REDUCING ZONES

One way analysis of variance probabilities. Data were untransformed. Oil was applied on day 60 in 1986.

	Creek Edge	Midmarsh	High Marsh
1987			
Day 45	0.284	0.162	0.000
Day 75	0.568	0.172	0.627
Day 110	0.162	0.786	0.571

APPENDIX 3: SUMMARY TABLE OF ANALYSIS OF VARIANCE RESULTS FOR PLANT
HEIGHT DATA

One-way analysis of variance probabilities. Data were transformed by $\ln((x/14)+1)$. Treatments were applied on day 60 in 1986.

	Creek Edge		Midmarsh	High Marsh
	Oil	Dispersant	Oil	Oil
1986				
Day 26	0.000	0.404	0.000	0.006
Day 34	0.001	0.000	0.128	0.425
Day 49	0.000	0.000	0.133	0.455
Day 57	0.041	0.692	0.245	0.000
Day 75	0.601	0.000	0.400	0.000
Day 89	0.043	0.000	0.020	0.000
Day 102	0.179	0.000	0.199	0.055
Day 123	0.034	0.000	0.024	0.000
1987				
Day 26	0.000	0.000	0.095	0.000
Day 48	0.000	0.021	0.001	0.486
Day 67	0.065	0.126	0.000	0.000
Day 90	0.351	0.785	0.000	0.000
Day 115	0.034	0.455	0.000	0.000

APPENDIX 4: SUMMARY TABLE OF ANALYSIS OF VARIANCE RESULTS FOR STEM
DENSITY DATA

One-way analysis of variance probabilities. Data were transformed by $\ln(x + 1)$. Treatments were applied on day 60 in 1986.

	Creek Edge		Midmarsh	High Marsh
	Oil	Dispersant	Oil	Oil
1986				
Day 26	0.875	0.000	0.625	0.046
Day 49	0.190	0.000	0.039	0.172
Day 89	0.000	0.000	0.000	0.000
Day 123	0.043	0.000	0.000	0.000
1987				
Day 26	0.477	0.000	0.000	0.848
Day 48	0.187	0.106	0.000	0.052
Day 67	0.168	0.000	0.000	0.021
Day 90	0.926	0.162	0.000	0.073
Day 115	0.014	0.031	0.000	0.019

APPENDIX 5: SUMMARY TABLE OF ANALYSIS OF VARIANCE RESULTS FOR STEM
DENSITY OF FLOWERING SPARTINA PLANTS

One-way analysis of variance probabilities. Data were transformed by $\ln(x + 1)$. Treatments were applied on day 60 in 1986.

	Creek Edge		Midmarsh	High Marsh
	Oil	Dispersant	Oil	Oil
1986	0.353	0.357	0.274	0.138
1987	0.134	0.004	0.130	0.525

**APPENDIX 6: SUMMARY TABLE OF ANALYSIS OF VARIANCE RESULTS FOR THE BIOMASS
DATA**

One-way analysis of variance probabilities. Data were transformed by square root.
Treatments were applied on day 60 in 1986.

	Creek Edge				Midmarsh		High Marsh	
	Oil 1986	Oil 1987	Disp 1986	Disp 1987	Oil 1986	Oil 1987	Oil 1987	Oil 1986
Standing Dead Biomass	0.022	0.400	0.397	0.054	0.009	0.144	0.841	0.184
<u>Spartina alterniflora</u> biomass	0.613	0.488	0.017	0.685	0.003	0.096	-	0.374
<u>Spartina patens</u> biomass	-	0.374	-	-	0.374	0.271	0.221	0.010
<u>Salicornia europaea</u> biomass	-	0.786	0.374	0.370	-	0.605	-	0.978
<u>Suaeda maritima</u> biomass	-	0.774	-	0.581	-	0.997	-	-
<u>Atriplex patula</u> biomass	-	0.374	-	0.374	-	-	-	-
Total Above-ground Living Biomass	0.613	0.480	0.017	0.627	0.003	0.115	0.221	0.011
Mean Weight Per Shoot	0.980	0.820	0.421	0.389	0.624	0.099	0.244	0.799
Total Biomass of Flowers	0.322	0.193	0.378	0.006	0.148	0.136	0.225	0.376

	Creek Edge		Midmarsh	High Marsh
	Oil	Dispersant	Oil	Oil
1987				
<u>Spartina alterniflora</u>				
Day 45	0.246	0.003	0.000	0.332
Day 56	0.004	0.000	0.000	0.332
Day 70	0.052	0.219	0.000	0.332
Day 90	0.150	0.395	0.000	0.332
Day 110	0.592	0.000	0.000	0.332
<u>Spartina patens</u>				
Day 45	-	-	0.167	0.011
Day 56	-	-	0.400	0.115
Day 70	-	-	0.346	0.001
Day 90	-	-	0.315	0.063
Day 110	-	-	0.157	0.031
<u>Salicornia europaea</u>				
Day 45	0.853	0.876	0.570	0.001
Day 56	0.672	0.346	0.788	0.278
Day 70	0.044	0.000	0.116	0.001
Day 90	0.023	0.000	0.146	0.000
Day 110	0.069	0.000	0.202	0.000
<u>Suaeda maritima</u>				
Day 45	0.276	0.002	-	1.000
Day 56	0.030	0.000	1.000	0.555
Day 70	0.188	0.000	0.044	1.000
Day 90	0.291	0.000	0.125	0.555
Day 110	0.310	0.000	0.011	0.470

APPENDIX 8: SUMMARY TABLE OF ANALYSIS OF VARIANCE RESULTS FOR PLANT
VARIABLE FLUOROMETRY DATA

One-way analysis of variance probabilities. Data were transformed by square root. Treatments were applied on day 60 in 1986.

		Creek Edge		Midmarsh	High Marsh
		Oil	Dispersant	Oil	Oil
100 Second Difference Values					
	Day 47	0.474	0.015	0.212	0.031
	Day 68	0.041	0.522	0.534	0.927
	Day 88	0.109	0.617	0.327	0.895
	Day 111	0.636	0.232	0.675	0.879
Peak Variable Fluorescence					
	Day 47	0.426	0.074	0.290	0.085
	Day 68	0.465	0.338	0.021	0.900
	Day 88	0.182	0.676	0.131	0.513
	Day 111	0.304	0.830	0.004	0.263

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Project E183

ENVIRONMENTAL EFFECTS MONITORING:
PITFALLS AND POSSIBILITIES IN RELATION TO
OFFSHORE OIL DEVELOPMENT

1989a



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EXECUTIVE SUMMARY

Canada's first offshore oil production platform will be installed on the Grand Banks, off St. John's, Newfoundland, within a few years. The developers of offshore oil will be required to conduct an Environmental Effects Monitoring (EEM) program in co-operation with the Canadian/Newfoundland Offshore Petroleum Board, Environment Canada and other government agencies. Some of the conceptual framework for such EEM programs is available, and it provides a basis for decisions on what to monitor and, in practice, how to monitor. The quantitative aspects of offshore EEM, however, have received less attention. This report discusses these quantitative aspects and, thus, supplies guidance for the designers of the offshore monitoring program.

The report's specific objectives are to review past approaches to EEM of offshore oil operations, to identify the pitfalls of these approaches with particular reference to their quantitative aspects, and to suggest possible directions for the initial EEM program. Chapters on these topics are prefaced by a discussion of EEM in Canada and an exploration of some statistical considerations. The latter is structured as an introduction, for non-statisticians, to topics covered in the review.

In Chapter 1, the need for a conceptual framework for EEM is outlined, as a basis for further discussion. The importance of fully developing such a framework before EEM program planning begins is emphasized. Several aspects of this framework are discussed, including the importance of hierarchical structure of biological organization, the nature of environmental change, and the importance of understanding causality in perturbed ecosystems.

This report adopts Environment Canada's definition of Environmental Effects Monitoring (Anon., 1986) and, thus, excludes from consideration related topics such as Surveillance Monitoring and Compliance Monitoring. It is suggested that, even within this definition, EEM has multiple objectives. Three types of EEM are defined: early warning EEM, verification EEM and experimental EEM. The former is intended to provide warnings of environmental degradation in time for corrective measures to be taken. Verification EEM is designed to verify that impact predictions were accurate. Experimental EEM is that form of monitoring carried out when a development is visualized as a research experiment centered on the responses of the environment to large-scale perturbation as the measurement phase of the experiment. The differences among these three types of EEM are discussed as are the differences among monitoring and research, and the role of the experimental approach to environmental management.

In Chapter 2, six major studies of the environmental effects of offshore oil operations are reviewed, together with a discussion of the large number of such studies carried out in the North Sea and a review of three Canadian proposals for offshore EEM. It is suggested that no EEM, as here defined, of offshore oil production has yet been attempted. The monitoring proposed for the first offshore oil development project is, therefore, novel.

Three major studies were carried out in the Gulf of Mexico in the 1970's: the Offshore Ecology Investigation, the Central Gulf Platform Study and the Buccaneer Gas and Oil Field Study. These were all a posteriori studies of the environmental effects of prolonged oil production. The quantitative design flaws that led each of them to inconclusive results are described. The research-oriented emphasis of the North Sea work is described. Quantitative methods have rarely been employed in that work. The other reviewed studies are: the Mid-Atlantic Block 684 Study, the first comprehensive EEM of an offshore exploration well; the Georges Bank Monitoring Program, an integrated regional offshore monitoring program; and the California Monitoring Program. The Georges Bank program included monitoring of one exploration well, but the cessation of oil activity in that region has prevented the program's intended progress into the monitoring of the oil production phase. The California Monitoring Program was designed to monitor oil production and has commenced its pre-development surveys, but further progress towards production drilling has ceased in that area. Meanwhile, two designs for an EEM program for Hibernia have been prepared, one on behalf of the Environmental Studies Revolving Funds and the other for Mobil Oil Canada Limited. The last initiative reviewed here is the Beaufort Environmental Monitoring Program, a planning and research co-ordination program designed to support EEM in the Beaufort Sea when offshore oil production commences there.

The designs of each of these various studies is examined as a model for EEM for projects such as Hibernia, and their quantitative flaws for that purpose are described. It is suggested that the sum of this experience of offshore environmental effects studies can provide assistance to the designers of the EEM programs for the Grand Banks.

In Chapter 3, a wide range of possible solutions to the pitfalls confronting quantitative offshore EEM are presented. Because EEM is relatively novel and because there is no successful experience on which to build a program design, these proposed solutions do not amount to such a design. They do, however, provide guidance for the designers of the Hibernia and other offshore EEM programs. The possible solutions fall into three groups: environmental management possibilities, statistical possibilities, and conceptual framework possibilities. It is suggested that the first step to be taken towards an effective EEM design is for the environmental managers to recognize the extent of the problems that the design must surmount. It is then shown that EEM design based upon experimental type objectives is unsuitable and that an alternative conceptual basis for EEM is needed.

The two further possibilities in the area of environmental management concern the formulation of explicit policy assumptions of the results of EEM. There are environments, environmental variables and potential impacts where the expected risk of degradation does not warrant the cost of monitoring, yet there is no scientific evidence to suggest that degradation does not warrant the cost of monitoring; and there is no scientific evidence to suggest that degradation cannot occur. It is suggested that the scope of EEM should be delimited by policy. Similarly, there are many situations in which EEM could establish that an environmental change has probably occurred but could not demonstrate that that change was an effect of the monitored development. In some of these situations, the cause could be set, for legal purposes, by a

policy decision. This would ease the task of EEM program designers without compromising the quality of environmental management.

No statistical analysis appropriate to offshore oil development EEM data is yet available but it is suggested that one could be developed from the General Linear Model, perhaps in combination with a Box-Jenkins time-series model. Non-parametric statistics have been previously suggested, but they do not offer any particular advantage for EEM.

In the past, monitoring data have been analyzed with null hypotheses of the general form: "there is no effect." While this is the correct form for a null hypothesis in research, it is suggested that it would be more appropriate for EEM analyses to use: "the effect is no worse than predicted in the environmental impact statement."

Possible station arrays and sampling frequencies are discussed, as is the value of geostatistics for spatial smoothing of monitoring data. Other statistical topics explored include the need for inference-testing analyses, the importance of Type II errors and the requirement for development of statistical models of EEM.

A good conceptual framework should include or provide the rationale for the following:

- 1) adequate scoping with explicit objectives and a clear distinction between the roles of science and policy.
- 2) a way to select variables that would span the biological hierarchy and yet be meaningful to monitor from environmental management perspective.
- 3) adequate bounding of space and time relationships of the development and the environment. Central to this is the notion of environmental change.
- 4) adaptive approach that will encompass as wide a range of human activities, both routine and accidental, related to the development as resources will permit.
- 5) methodology to frame the understanding of cause and effect from a development activity through the biological hierarchy to the ecosystem and this is best accomplished through a cluster of models, perhaps centered on the concept of environmental risk.
- 6) awareness of the quantitative pitfalls and their solutions culminating into strong sampling designs and appropriate data analysis and interpretation.
- 7) awareness of how the results of EEM can be made most useful for environmental management including other forms of monitoring assessment, public participation, protection planning, contingency planning and, in particular, area wide assessment and cumulative effects assessment and management, and policy decision framework. This should include a clear definition and understanding of important environmental change.

8) attention to the various logistical realities.

Several concrete suggestions for these aspects of the conceptual framework are provided.

We conclude with four recommendations as follows:

Recommendation 1: Develop the conceptual framework for the initial offshore oil development project using the eight aspects given in this report as a guide. These aspects need to be examined to a level similar to that done here for quantitative considerations.

Recommendation 2: Evaluate the trade-offs of the various possibilities for EEM design in terms of their ecological effectiveness and costs to monitor. This is not an exercise in precision but rather "informed decision making", which should culminate in some practical decisions for offshore EEM.

Recommendation 3: Using the results of Recommendations 1 and 2, develop the quantitative (theoretical) aspects of the EEM design including both statistical and mathematical modelling and determine the required field data.

Recommendation 4: Using the results of Recommendation 3, plan the implementation of the field studies that are needed for the quantitative aspects of the EEM design. The lead-time for EEM at Hibernia, for example, is now minimal, and we recommend urgent action on program development.

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1.0 INTRODUCTION

1.1 Background

1.1.1 Monitoring and the Hibernia Oil Development

In May 1985, Mobil Oil Canada Ltd. and its partners submitted an Environmental Impact Statement (EIS) concerning their intended offshore oil development at Hibernia, on the Grand Banks of Newfoundland (Mobil Oil Canada Ltd., 1985). They proposed to construct a production platform, either gravity based or floating, with its associated production manifolds, satellite wells and pipelines on the seabed. The produced oil would be taken ashore by shuttle tankers, so the development would also require tanker moorings and, for a floating platform only, a moored floating storage tanker. Neither production platform would require a pipeline to land. Approximately 100 development wells would be drilled, and more than 100,000 barrels per day of crude oil would be produced. This production would continue for approximately 20 years (Mobil Oil Canada Ltd., 1985).

The Hibernia Environmental Assessment Panel was convened by the Federal Environmental Assessment Review Office to review this EIS and to conduct public hearings to determine if and how the project could proceed in a safe and environmentally acceptable manner. The Panel tabled its report in December 1985 and concluded that the project could proceed. They made several recommendations, however, including a key stipulation that Environmental Effects Monitoring (EEM: defined in Section 1.2) should be undertaken throughout the twenty year duration of the project. They noted that:

"Several written submissions to the Panel severely criticized Mobil's approach to the subject of effects monitoring, stating that the plans should have been more specific. The interveners considered that sub-lethal effects should be monitored, that fish and birds should be included in the studies [in addition to sessile benthic organisms], and that effects of episodic events should be addressed as well as long term impacts." (Paradine et al., 1985, p. 39)

Mobil responded to these criticisms by stating its support for the findings of a recent Environmental Studies Revolving Funds report on EEM (LGL Ltd. et al., 1985). The oil company proposed to develop the final monitoring design in consultation with the appropriate government agencies. The Panel therefore recommended that:

"A monitoring plan should be developed taking into account the viewpoints of the responsible government agencies and allowing for publication of results. The government agencies should consult with interested public groups concerning the monitoring program." (Paradine et al., 1985, p. 40).

Although several preliminary steps have been taken towards the design of that monitoring plan, low world oil prices since 1985 have caused an indefinite postponement of the development of Hibernia. Plans for monitoring that development have, in consequence, also been delayed. On 18 July 1988, however, the Government of Canada, the Government of Newfoundland and Labrador, Mobil Oil Canada Ltd. and its partners signed an agreement to proceed jointly with the development. The current design largely follows that outlined in the EIS, with one significant modification: plans for a floating production platform have been cancelled and a gravity based structure is now the only option considered. The new development schedule calls for onshore construction to begin early in 1989, for the first produced oil to come ashore in 1995 and hence for the platform to be operational until about 2015.

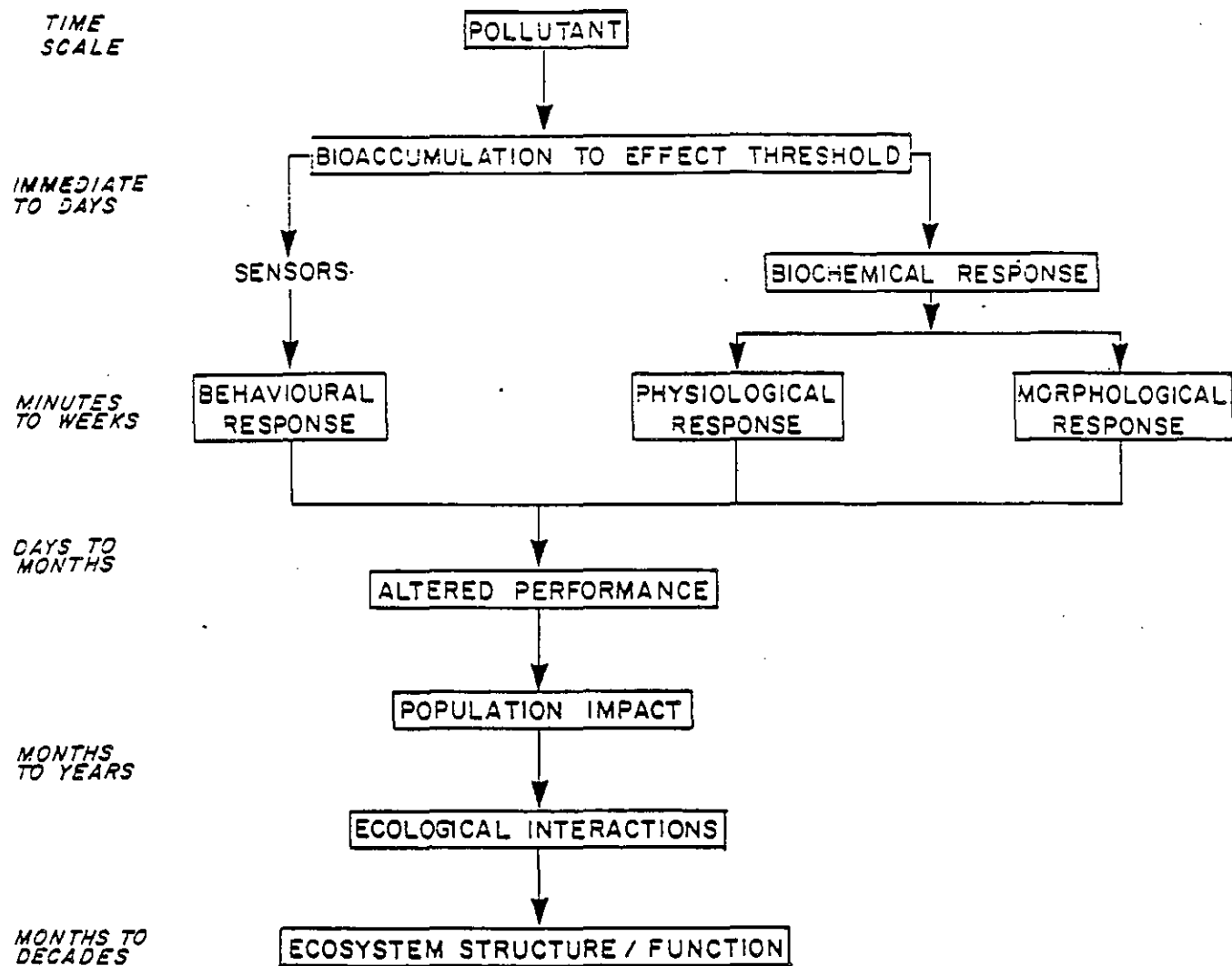
It is anticipated that the design of the EEM program called for by the Hibernia Environmental Assessment Panel will be synchronized with this new development schedule.

1.1.2 Developing a Conceptual Framework for EEM

Regardless of how the EEM program is designed, there should be an underlying conceptual framework. Often in EEM planning, there is not enough attention given to this framework and this has led to design flaws. A collection of variables to be measured may be selected on the basis of logistical convenience rather than biological significance. Statistically-based sampling designs are often lacking and quantitative rigor and quality control may be ignored altogether. Often, the causal relationships among the variables are not understood or described.

To design a conceptual framework for EEM of a development such as Hibernia, three basic points must be emphasized: 1) the biological hierarchy; 2) the nature of environmental change; and 3) conceptualization of causality and interrelationship at the ecosystem level. First, the causal relationships linking the development activities (chronic oil discharges, shipping, etc.) to potential effects in the biota must be viewed in a hierarchical way. Biological systems by their nature are hierarchically arranged and this influences all causal relationships among the biota and other aspects of the environment.

Figure 1.1 illustrates how this biological hierarchy is affected by a pollutant and the expected effects on each level of the hierarchy. If an effect is to be observed for any particular level of the hierarchy (for example, the population level), then the appropriate field, laboratory and quantitative methodologies must be in place to detect the change. In addition, the causality from the discharge of the pollutant (or other activity) into the environment to the effect must be understood and at least plausible, if all causal links are not fully documented. Generally, as one proceeds from the lower levels of the biological hierarchy to the higher ecosystem level, cause and effect become more difficult to identify and document, and non-specific system response are observed. This non-specific system response may be causally related to many sources both natural and anthropogenic ones. Furthermore, the causality is more difficult to document for chronic routine pollutant discharges than for major oil spills.



Source: after Gesamp (1980) and Scope (1984).

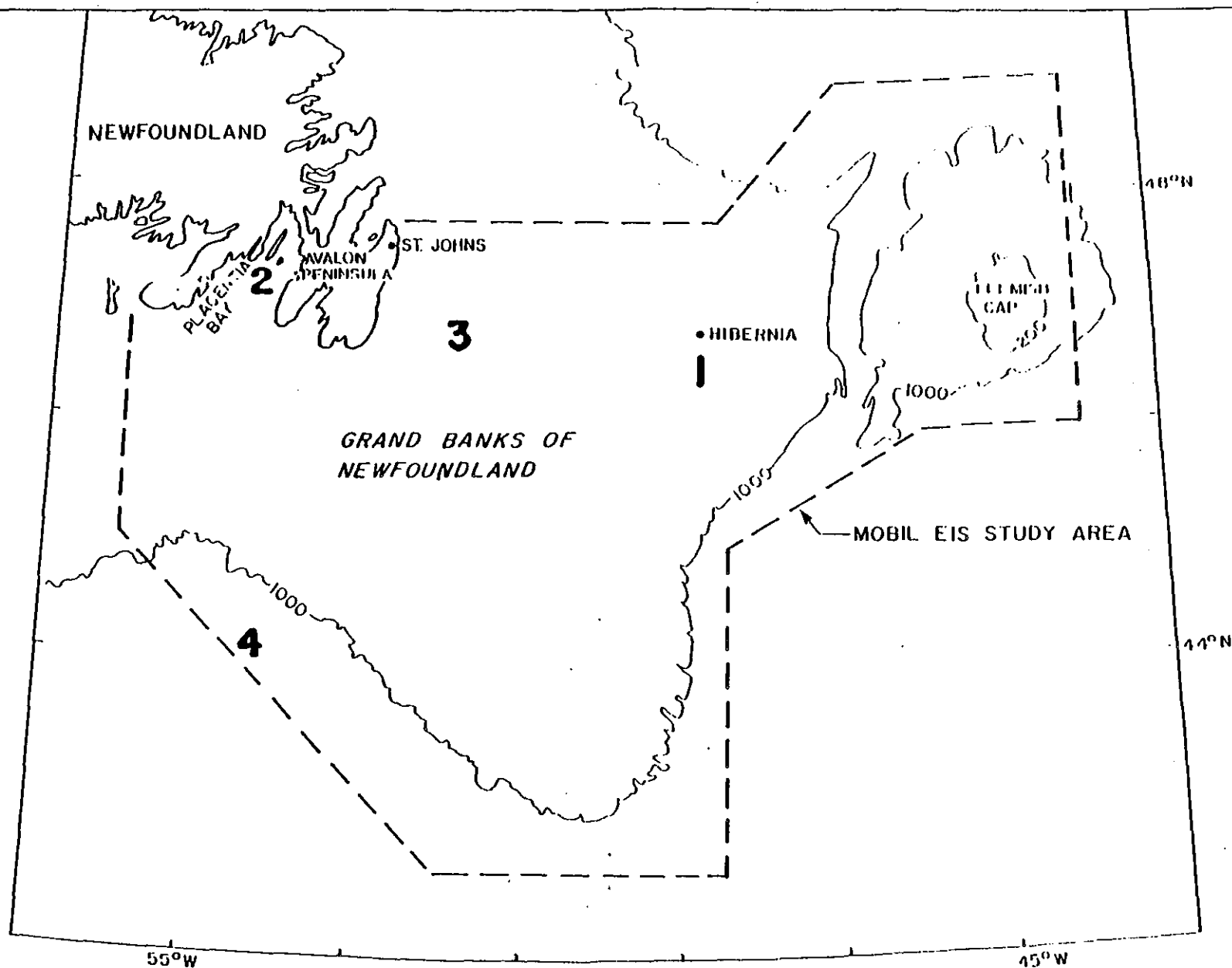
SCHEMATIC DIAGRAM OF THE EFFECTS AT
DIFFERENT LEVELS OF THE BIOLOGICAL
HIERARCHY AFTER INTRODUCTION OF POLLUTANT
INTO THE ENVIRONMENT

For example, the conceptual framework of the Hibernia EIS prepared by Mobil Oil Canada Limited, involved the population level of the biological hierarchy only and in a somewhat restricted sense. Standing crop and not turnover rate or standard demographic descriptors (r,b,d, age structure, etc.) were employed. Effects can occur on every level of the biological hierarchy from the sub-cellular to the ecosystem. By concentrating on the population level assumptions may have been made that then influenced their results of "no or negligible impact". In particular, by using standing crop, they concluded that natural variability was so large that most population estimates cannot be determined with certainty or that no detectable effects can be found between natural and perturbed levels of population change. P. Lane and Associates (1985) in contrast, using a population level risk analysis involving a stochastic Leslie matrix for a hypothetical murre population, found that for chronic impact scenarios at Hibernia, mean population declines of 11-50% were possible. An increase of adult mortality by 86% over 60 years could seriously threaten population survival. This result assumed strong density-dependence was operative. For a population with weak density dependence, it could take 46 years to return to 90% of the equilibrium value. This period is more than twice the expected life of the Hibernia development. These results also suggest that routine activities and chronic discharges can cause environmental effects of concern. For major oil spills at sensitive parts of the murre life history such as their swimming migration, the population could be seriously affected. Dr. R. Brown (pers. comm.) has cautioned, however, that there is no direct and simple relationship between amount of oil spilled and ecological damage unless a large number of related factors are considered. Thus small oil spills are not necessarily inconsequential to ecological systems.

The concept of EEM centers on the detection of change. Change in and of itself is universal in ecological systems. To document change, there must be a baseline and a time (or set of times) in the future when a change is to be observed. Often, absolute change may not be as important as pattern of change. Usually some sort of inferential statistical analyses must be invoked to determine the probability of whether the change is "real".

Change is a relative concept, since the magnitude of change must always be related to a particular space and time framework (P. Lane and Associates, 1985). In a biological hierarchy such as that given in Figure 1.1, change occurs at different temporal and spatial scales. For example, biochemical and physiological responses may take a few minutes to a few weeks whereas alterations in ecosystem function may take months to decades (SCOPE, No. 22, 1984). It is also not enough to demonstrate change, it must also be shown that the observed change is causally related to the development activities.

With a project such as Hibernia, various spatial scales are also important. Figure 1.2 illustrates three areas that are expected to be impacted: (1) drilling area, (2) land-based facilities, and (3) shipping routes. Area 4 encompasses the entire area discussed in Mobil Oil Canada Limited's E.I.S. Because of the differences in development activities in each of these areas, associated sampling methods and quantitative methodologies for EEM would also be different.



POTENTIALLY IMPACTED AREAS OFF THE COAST OF NEWFOUNDLAND AS OIL DEVELOPMENT PROCEEDS AT HIBERNIA (1) DRILL SITE (2) SHORE-BASED ACTIVITIES (3) SHIPPING SITES (4) E.I.S. STUDY AREA

If the appropriate spatial scale is not selected for a particular type of development activity and its potential effect, then a biased conclusion may result on the importance of the observed changes and they may not appear to be real effects causally related to the development. For example, in sampling changes in benthic community diversity around the drill site, a scale interval of 5 m may be too small and a scale of 5 km would be too large. Measures of change would be very different at those two scales.

The way causality and interrelationship are conceptualized is critical to the success of any environmental effects monitoring programme. To date, much of the management of complex systems has failed because of the failure to understand interconnection (P. Lane and Associates, 1985, Lane, 1986a). This is discussed in more detail in Section 4.3

A good conceptual framework should include, or provide the rationale for the following:

- 1) adequate scoping with explicit objectives and a clear distinction between the roles of science and policy.
- 2) a way to select variables that would span the biological hierarchy while being meaningful to monitor from environmental management perspective.
- 3) adequate bounding of space and time relationships of the development and the environment. Central to this is the notion of environmental change.
- 4) an adaptive approach that will encompass as wide a range of human activities, both routine and accidental, related to the development as resources will permit.
- 5) a methodology, to frame the understanding of cause and effect from a development activity, through the biological hierarchy to the ecosystem. This is best accomplished through a cluster of models, perhaps centered on the concept of environmental risk.
- 6) an awareness of the quantitative pitfalls and their solutions culminating in appropriate sampling designs and data analysis and interpretation,
- 7) an awareness of how the results of EEM can be made most useful for environmental management including the integration of EEM with other forms of monitoring assessment, public participation, protection planning, contingency planning and, in particular, areawide assessment and/or cumulative effects assessment and management, and policy decision framework. This should include a clear definition and understanding of important environmental change.
- 8) attention to the various logistical realities:
 - manpower
 - field and laboratory methods and equipment
 - quantitative methods (statistics and modelling)
 - reporting structure (data management systems)
 - budget

1.1.3 Related Literature

There is a large body of recent literature on environmental effects monitoring (EEM) which will, collectively, form the scientific foundation of the first offshore oil development project monitoring plan. Much of this literature is reviewed elsewhere in the present report.¹ A few items are, however, of particular importance. Conover (1985) and Barchard and Mocres (1988) have outlined two views of the nature and role of EEM, while Beariatis and Duinker (1983) and Munro et al. (1986) have each attempted to place EEM within the wider context of environmental impact assessment and environmental management. These reports are not in full mutual agreement. The above reports have all been broadly conceptual.

The design of a practical EEM program will require more detailed advice in at least three areas: the qualitative question of what to monitor, the practical question of how to measure it, and the quantitative questions of when, where and how often to measure it. What to measure has been addressed by a great number of studies, summarized most recently, but with no particular focus on the EEM of offshore oil production, by Maritime Testing (1985) Ltd. (1987). How to measure the chosen variables is the subject of such practical experience but has received less attention in the literature. Environment Canada (1987) does, however, provide a detailed description of methods appropriate to one Canadian marine environment, the Arctic.

The remaining, quantitative question forms the focus of the present report. As is shown below (Chapter 2), this question has been neglected in the past, despite much of the necessary statistical background being clearly enunciated a decade ago by Eberhardt (1976). His comments have recently been taken up again by Millard et al. (1985), Millard and Lettenmaier (1986) and Millard (1987a,b). These recent papers have been largely theoretical, however, and directed to EEM of power plants, not of offshore oil platforms. The only published work directly comparable to the present report is Carney's (1987) recent and perceptive review of the quantitative and design aspects of EEM of offshore oil installations in U.S. waters. That paper closely parallels the present work. Many of Carney's (1987) points are discussed herein, but to avoid excessive repetition the present report is designed to complement, rather than replace, his earlier review. This report should, therefore, be read in conjunction with Carney's (1987) work.

There have been two attempts to turn some of these, sometimes rather abstract, ideas into a practical monitoring plan for Hibernia. Shawmont Martec Ltd. and E.G. & G. Environmental Consultants (SMEGG, 1983) prepared a draft plan for Mobil Oil Canada Ltd., while LGL Ltd. et al. (1985) included another plan with their proposed strategy for EEM of oil production off the east coast of Canada. Both are reviewed elsewhere in this report (Sections 2.4.1 and

¹ Descriptions of the nature of oil production platforms and of the types and amounts of contaminants released from them can be found in papers by Davies et al. (1981, 1984), Dicks (1982) and Menzie (1982).

2.4.2). Neither adequately addressed the quantitative problems associated with offshore EEM.

Since the focus of the present report is on the quantitative aspects of EEM only, when designing the EEM program for offshore Newfoundland, it should be used conjointly with all of these earlier reports and publications.

1.2 Environmental Effects Monitoring (EEM)

1.2.1 Nature of EEM

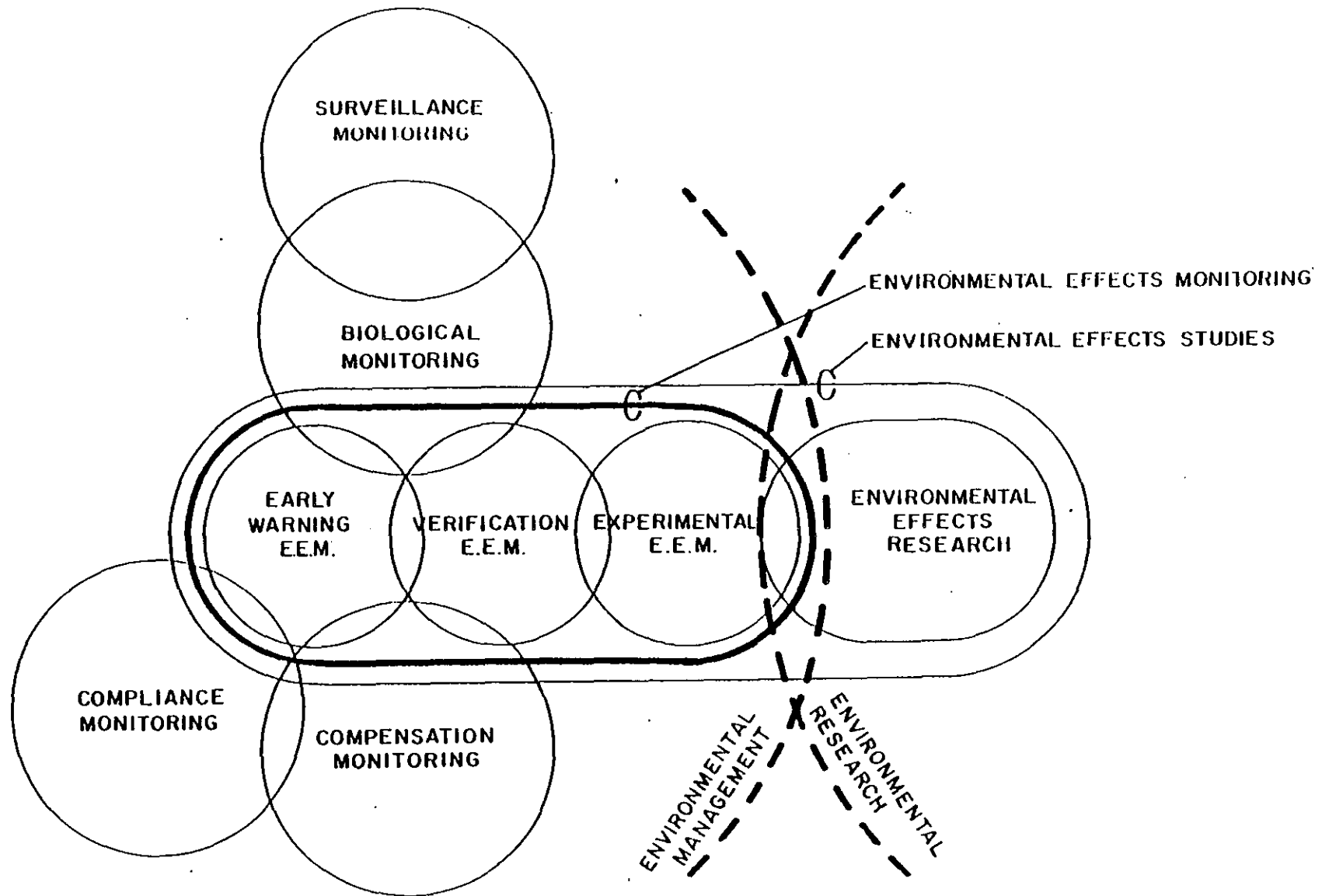
Environmental management procedures must include programs for the routine monitoring of many variables, from the levels of various anthropogenic inputs to the environment to the impacts of changes in that environment on human affairs. This wide variety of variables to be monitored leads to an equally wide range of types of monitoring programs. Although there is no consensus on a classification of those types (Meijers, 1986), several are widely recognized (Figure 1.3). "Compliance Monitoring", for example, involves the recording of contaminant discharge rates at the end of an outfall pipe, whereas "Surveillance Monitoring" requires regional surveys of the concentrations of contaminants in the environment, without direct reference to their sources (Conover, 1985; Barchard and Moores, 1988).

Within this range of activities, "Environmental Effects Monitoring" (EEM) is intended to measure the effects of particular anthropogenic inputs on the environment, and more specifically on the biotic or, at most, the biophysical environment. For this purpose, an "effect" is usually a change in some environmental variable, measured either in space or time, that can be attributed to an anthropogenic causative factor. EEM has therefore been defined by Environment Canada (Anon., 1986) as:

"A program of observations for the purpose of determining whether the presence, or change in the incidence, of a factor(s), has adversely affected critical biological processes, or the physical and/or chemical nature of a system. (The effect so detected should be attributable to a specific action, agent or process of man.)"

This definition is followed in the present report with some latitude appropriate to a theoretical (rather than operational or regulatory) context, of the stress on "adverse" effects and "critical" processes.

In a recent proposal for an EEM strategy for east coast Canadian offshore developments, LGL Ltd. et al. (1985, p. 11) restricted the definition of EEM by confining it to the study of chronic effects. In common with the Hibernia Environmental Assessment Panel, we suggest that a complete EEM program should include provisions for monitoring episodic events associated with the development, for example, a blowout or major spill resulting from the failure of an on-site oil storage system. There is, however, the need for EEM to assign causes to any detected effects, as required by the above



A SCHEMATIC CLASSIFICATION OF THE THREE TYPES OF ENVIRONMENTAL EFFECTS MONITORING AND RELATED ACTIVITIES (ADAPTED FROM BARNARD AND MOORES, 1988)

definition and as emphasized by both LGL Ltd. et al. (1985) and Maritime Testing (1985) Ltd. (1987).

EEM, as thus defined, includes neither Surveillance Monitoring, which lacks regard for specific human actions, nor Compliance Monitoring, which is concerned only with "factors" and not with their effects. An overall monitoring program must include other types of monitoring as well as EEM, but only the latter is considered in this report. EEM is concerned only with effects on the natural environment and not with effects on human affairs. For example, the construction and operation of an offshore oil platform can have a direct effect on a fishery by excluding fishing activity from a safety zone around the platform. In theory, it might also affect the fishery by reducing the productivity of the fish resource through the discharge of contaminants. EEM as defined above includes monitoring designed to detect the reduction of resource production, but not for the detection of the social and economic impacts either of any such reduction or of the exclusion of fishing activity from the safety zone.

A further distinction may usefully be drawn between EEM and "biological monitoring". While the latter term is sometimes (Dalby, 1979) used to include the monitoring of biological variables (often, but not always, an aspect of EEM), biological monitoring is more commonly the use of biological systems to monitor chemical contaminant levels, for example, body burden measurements (Baker, 1976). This approach has been extensively used, with considerable success, but it is not usually EEM. Biological monitoring programs are not generally designed to detect the effects of the chemical distributions that they measure and, in consequence, they do not detect such effects.

If EEM, as here defined, has a narrow focus, it is nevertheless potentially an important tool for environmental management. Although the ultimate purpose of such management may be to optimize the human benefits of development, that purpose is pursued by attempting to optimize the condition of the human environment. It follows that the most direct way to measure the success, or otherwise, of environmental management programs is to monitor that condition (Dalby, 1979). When performed in relation to a particular source of anthropogenic environmental stress, this direct monitoring of the immediately important environmental factors is provided by EEM (McIntyre et al., 1978; Gray, 1980; Gray et al., 1980; McIntyre, 1984, 1986). Furthermore, all predictions of environmental effects based on either conceptual models or extrapolation from laboratory or mesocosm studies must, ultimately, be verified by field measurements (Hartley, 1982; Boesch, 1984). That verification requires EEM.

The power of EEM, not to mention the quantitative aspects of its use does, however, depend on the chosen definition of an "effect". To the production engineer, the distribution of chemical contaminants in the environment might reasonably be considered to be the "environmental effect" of the discharges that he or she has measured at the end of the outfall pipe. In contrast, to the biologist this distribution is merely the site-specific expression of the contaminant stress, and hence an aspect of the "cause", not of the "effect". Even among biologists, there can be major

disagreements as to the proper definition of an "effect". Toxicologists may detect some chemical change in animals exposed to contaminants [induction of Mixed Function Oxidase (MFO: Payne, 1976, 1985)] while ethologists may see some behavioral response to pollution, such as a movement out of an effluent plume. Each of these is, in a very real sense, an "effect" of the relevant anthropogenic input. To the ecologist, however, neither are environmental effects but rather are "responses"; factors on the pathway between the chemical stress and its ultimate effect. The induction of MFO, for instance, may serve to rid the animal's tissues of a contaminant which cannot then cause any harm. The movement of a fish out of an effluent plume may isolate it from contamination, without being in any way disadvantageous. An ecologist is more likely to see an "effect" as some change in the parameters of population dynamics in one or more species, or as some alteration of the inter-relationships between those species, in the impacted ecosystem.

In this report, we largely concentrate on the hierarchical view of environmental effect, as illustrated in Figure 1.1, for two reasons. First, the power and purpose of EEM stem from its (potential) ability to measure changes in variables of ultimate interest to mankind. Measurements of small scale movements of fish are unlikely to be of much interest to the general public, whereas the failure of reproduction of an exploited population would be of great economic importance. Eventually, EEM must be able to address the population and ecosystem levels of the hierarchy.

The second reason for the present emphasis on higher-level effects is that they pose the major quantitative problem for EEM. Carney (1987) has pointed out that the planners of most EEM programs have implicitly assumed that the effects they sought to measure would be "pathological"; that is, something would be observed that could only be an effect of the anthropogenic stress of immediate concern. MFO induction offshore may indeed be a "pathological effect" in the sense of Carney (1987). If so, monitoring for MFO would be simply a matter of gathering samples and performing assays. Any quantitative analysis need not be complex. Many environmental effects are, however, "discordant" (*sensu* Carney, 1987) rather than "pathological"; that is, they take the form of a percentage change in a variable, which would be constantly changing in the presence of natural variation whether the anthropogenic stress were applied or not. Since a monitored production platform is unlikely to be the sole source of hydrocarbons, even in its own offshore area, one might expect some background level of MFO induction, preventing identification of a clear causal relationship with the development platform. Monitoring for "discordant" effects demands a quantitative, and specifically a statistical, approach and so that is the type of EEM of principal concern here.

1.2.2 Types of EEM

Even within the restricted definition used here, EEM can have any one or more of several distinct objectives. These can include:

- a: to provide early warning of undesirable change.

- b: to improve environmental understanding, especially of cause-effect pathways,
- c: to verify impact predictions,
- d: to check effectiveness of mitigation measures, and
- e: to satisfy regulations and public concerns.

Although these objectives are superficially similar, they require quite different EEM approaches and hence three, more or less distinct, types of EEM can be recognized.

Early warning (objective a) is essentially predictive and anticipatory. It must involve frequent, rapid measurements of simple variables and some form of model which predicts what ultimate effects will arise if present trends in contaminant discharge, or other input factors, continue. Early warning will often be partially based on Compliance Monitoring, but it can be more efficient and more reliable to also monitor the primary effects of those contaminants on the bio-physical environment.

A quite different approach to EEM is implicit in objective (b). This approach, discussed in more detail in Section 1.3, treats the monitored development as a scientific experiment in environmental perturbation and EEM as its measurement and analytical phases. Leaving aside the validity of the concept, this approach can only have value as scientific research, rather than as an operational activity. Monitoring for this objective is, therefore, a purely "scientific" process and should meet the normal standards for scientific publication, which will be more rigorous than those required for early warning. This "experimental" EEM will also always be retrospective, rather than predictive, and will usually be concerned with the secondary or higher level effects of contaminants, since the primary ones are often better studied by laboratory experimentation. As such, experimental EEM will be quite different to early warning EEM in its practical aspects. There are also important conceptual (see Sections 1.3, 2.4 and 3.2) and analytical (see Sections 3.4 and 5.4) differences between these two types of EEM.

Objectives (c) and (d), verifying the effectiveness of mitigation and the accuracy of the predicted impacts, involve some of the attributes of each of experimental and early warning EEM. In consequence, the field and analytical aspects of such "verification" EEM will, in part, be some combination of those already discussed. Verification EEM is, however, primarily a management, rather than research, task. As such, some of the scientific rigor required in experimental EEM can be relaxed for verification EEM, as explained in later chapters.

Finally, objective (e), EEM to satisfy regulations and public concerns, combine scientific and non-scientific elements. Clearly, there is a real need for all monitoring activities to fit into a larger political and legal context and, where the regulations are themselves framed around one of the preceding objectives, satisfying the regulations will require only minor modifications to other EEM designs.

In this report, each of early warning, verification and experimental EEM is discussed regarding their pitfalls and their possible solutions. Each type of EEM is, however, as different as their objectives. The characteristics of each type therefore form one basis for subsequent discussion in this report.

1.2.3 Distinction Between Monitoring and Research

One final clarification of the scope of EEM is essential. In this report, the term "environmental effects study" will be used to encompass all scientific (in the broadest sense) activities designed to elucidate the effects on the environment of an anthropogenic stress. These studies fall into one of two broad classes: "environmental effects research" and EEM. The difference starts as a matter of purpose and intent, but produces fundamentally different types of studies.

Environmental effects research usually starts with a question: "What is the effect of an oil production platform on macrobenthos?" "How far are drill cuttings dispersed?" "Why is MFO induced in fish exposed to hydrocarbons?". These questions lead to specific research projects and thence to relatively clear-cut answers which serve to build a general body of knowledge. The answers can be clear-cut precisely because the questions can be clearly phrased and highly specific. That very specificity, however, makes the ultimate knowledge base more of an open framework than a solid foundation. Application of this knowledge to future environmental management must rely on interpolation between and extrapolation beyond the specific research results.

EEM, in contrast, begins with a directive ("Measure the effect of this oil production platform on macrobenthos in such a way as to detect any abundance change of more than 25%"). This leads to long-term, broad-scale monitoring programs which cannot provide the succinct results of a research program. EEM should, however, be able to show whether or not a change of the appropriate magnitude has occurred. This requires quite different approaches to quantitative analysis, statistical testing and logical deduction from those appropriately employed in research.

The distinction between environmental effects research and EEM is somewhat blurred. This distinction is, nevertheless, important in the design of effects studies and will be referred to extensively in this report. It has been recognized in the monitoring literature for several years (Thomas et al., 1981). Recognition of this difference between monitoring and research has an important corollary; monitoring is a facet of environmental management, not of environmental science. This position is widely accepted in the monitoring literature (Hecky et al., 1984; Larkin, 1984; Munro et al., 1986; Ter Haars and Meelis, 1986; Wolfe, 1987), but, as discussed in Section 1.3 below, an opposite view is becoming established in Canada.

1.3 The Role of Hypothesis Testing in Canadian Environmental Impact Assessment Procedures

The need for some types of EEM within the purview of environmental management is readily understood. Some test that impact predictions were realistic, coupled with a system for early warning of unanticipated severe impacts, is clearly desirable. The need for experimental EEM is less obvious. As is discussed in subsequent sections, effective experimental EEM faces more statistical problems than do the other forms of environmental monitoring. To explain this report's emphasis on the quantitative aspects of EEM, an outline of the place of monitoring and research within Canadian environmental assessment and management is given below.

Although the value of ecological monitoring has been recognized by some workers since the late 1950's (Cowell, 1978), the early Canadian impact assessments of the mid-1970's rarely invoked monitoring of any kind. Compendia of EIA techniques in that decade made little reference to monitoring; Lee and Wood (1978), for example, only recommended "post-auditing" of "a sample of previously approved developments". The EIS guidelines produced by Environment Canada in those years followed a similar view: in 1976, the MacKenzie River Dredging and Fraser River Ship Channel guidelines both stated "plans for monitoring... should be described", but the EIS's for the Eldorado Nuclear Uranium Refineries, the Haines Road - Alaska Highway Reconstruction and the Gull Island and Muskrat Falls Power Generation Sites were not required to mention monitoring.

The various deficiencies of these first assessments, however, were noted (Schindler, 1976), as was the need for an increased monitoring effort (Morak and Olsen, 1980) and, Holling (1978), in a major theoretical advance, stressed the need for environmental management structures capable of adapting to and learning from the environmental effects of earlier developments. Such structures must include feedback mechanisms, so that future decisions can benefit from past experience. Then, in the early 1980's, a trio of critical reviews suggested several requirements for future work in Canada (Rosenberg *et al.*, 1981; Beanlands and Duinker, 1983; Larkin, 1984). Each of these latter stressed the need for more post-development monitoring.

Rosenberg *et al.* (1981) suggested that impact assessments would benefit from the application of the scientific method (in a broad sense), including the use of monitoring: (a) to test the impact prediction methods and the predictions themselves, (b) to identify unexpected excessive impacts and those higher-order cumulative and indirect effects of the development project which cannot readily be predicted, and (c) to prepare a case history of the development, hence building a body of knowledge to aid future impact assessments. Monitoring would clearly involve both verification and experimental aspects.

Rosenberg *et al.* (1981) also noted several specific scientific failings of impact assessment methods as then practiced. Of present importance, they remarked on "a lack of hypothesis testing" and the absence of "experimental

manipulation and/or modelling". It is not entirely clear from their published work, but these comments appear to relate less to holistic perturbed ecosystem experiments (see below) than to experiments designed to investigate the primary effects of anthropogenic factors (the effects of spilled oil on a captive marine mammal, for example) as a background to impact assessment; for example, they specifically recommended that "use of statistical techniques in the design and analysis of e.i.a. [sic] research be improved" (Rosenberg et al., 1981, p. 618; our emphasis). They did, however, recommend that environmental impact assessment should be considered and structured as a research activity. While this was an understandable reaction to the standard of many of the impact assessments that Rosenberg et al. (1981) reviewed, it appears that they went too far. They too seem to have withdrawn from this idea. At least, they later stated: "Prediction making must not become an end in itself... predictions should be recognized as planning aids..." (Hecky et al., 1984, p.731).

Larkin (1984), in a review prepared in 1982, made generally similar observations to those of Rosenberg et al. (1981). He also stressed the need for properly-conducted pilot experiments and monitoring of developments both during and after the construction phase. He did suggest, however, that environmental impact assessment and scientific studies of disturbed ecosystems should be conducted as separate, but parallel, activities. This recognition of the distinction between assessment and research, so contrary to Rosenberg et al.'s (1981) view, led Larkin (1984) to several insights, such as suggesting that fewer resources be devoted to imprecise predictions of impacts and more to monitoring those impacts and responding quickly to protect the environment when necessary.

Coincident with these largely theoretical reviews, Beanlands and Duinker (1983) prepared a review of the state of environmental impact assessment in Canada, specifically as a framework of recommendations for future assessment procedures. Among many valuable suggestions, they included calls for more quantitative approaches, for more pilot-scale perturbation experiments and particularly for the inclusion of thorough monitoring programs as integral parts of the assessment process. They also stated:

"There is a growing conviction that the project must indeed be considered in an experimental context in which post-project monitoring is required to test the hypotheses (the impact predictions)." (Beanlands and Duinker, 1983, p. 7)

They expanded on this comment elsewhere (their pp. 58-60) and built upon it the idea that impact predictions should take the form of testable, or falsifiable, hypotheses (their pp. 85-87), following the Popperian² view of

² Karl Popper is one of the foremost philosophers of science. He is most famous for linking the Scientific Method with the idea of a "falsifiable hypothesis". In oversimplified summary, he suggested that no hypothesis can be proven by science; an hypothesis can only be disproven. A scientific hypothesis can then be distinguished from an unscientific

the "Scientific Method". Thus, Beanlands and Duinker (1983) produced a valuable, multifaceted framework for impact assessment, but within that framework they took Rosenberg *et al.*'s (1981) extreme view, that assessment be structured as a research activity incorporating the "scientific method" (*sensu lato*), and narrowed it to the point that an assessment would be a strict, hypothesis-testing experiment of holistic ecosystem responses to perturbation, incorporating the "Scientific Method" *sensu stricto*. (Whether or not that method is even appropriate to environmental research will be discussed in Chapter 3 of this report.)

Beanlands and Duinker's (1983) idea, as defined in the above quote, has merit. Ecosystems contain complex positive and negative feedbacks that make overall system responses to perturbation impossible to predict from knowing only direct links between pairs of variables. If predictive models are prepared from pilot-level studies, they still require validation by full-scale experimental perturbation of the ecosystem. Such perturbations are, in general, far too expensive to carry out purely for research. Thus, if the scientific understanding of perturbed ecosystems is to be advanced, it is essential that as much information as possible is gathered from the perturbations associated with development projects. Hence, there is an apparent need for the experimental approach to impact studies, and for experimental EEM as the measurement phase of the "experiments".

Conversely, environmental management has other, and more directly valuable, objectives than the acquisition of scientific knowledge of perturbed ecosystems. These other objectives require early warning EEM and verification EEM, along with other monitoring activities. If the requirements of experimental EEM included those of early warning EEM and verification EEM, and if experimental EEM were itself straightforward, it might be possible for some scientists to view a development as did Beanlands and Duinker (1983). Simultaneously, environmental managers and developers could regard it quite differently, and yet have a single, simple EEM program fulfill each group's needs. In practice, the three types of EEM are largely discrete and the needs of each must be incorporated in a complex overall EEM program. Furthermore, scientifically acceptable research EEM is difficult and expensive.

In short, experimental EEM and the experimental approach to impact assessment have an important place in the future of environmental management in Canada. Larkin's (1984) view was that these scientific activities should be separate from, though parallel to, environmental management and its supporting monitoring activities.

hypothesis, because the former can be disproven or falsified while the latter cannot be. The "Scientific Method" is an iterative sequential process of inductive thinking, erecting a falsifiable hypothesis, subjecting it to a critical test, and returning to more thinking. Many philosophers, scientists and others consider this the only true Scientific Method. In this report, it is termed the "Scientific Method" or associated with Popper's name.

Munro et al. (1986, p. 11) state: "EIAs are not made to advance science, even though the knowledge obtained in the course of assessments may have that effect." See also Hecky et al. (1984). Beanlands and Duinker's (1983) recommendations, including their stress on the testing of falsifiable hypotheses, however, are beginning to form the basis for much Canadian environmental impact assessment work. At least one recent set of Federal Environmental Assessment Review Office (FEARO) guidelines has been founded on those recommendations (FEARO, 1987). Equally, a very large effort has been devoted to the Beaufort Environmental Monitoring Project (BEMP), which is essentially an exercise in hypothesis formulation with the implicit assumption that the hypotheses are testable (LGL Ltd. et al., 1985; Indian and Northern Affairs Canada and Environment Canada, 1985). Finally, Conover (1985), reporting the views expressed at a suite of Environment Canada workshops, defined EEM as essentially a research activity. This is incorrect. Thus, there is a danger in attempting to equate two very different types of activity, namely doing science and conducting EIA's.

1.4 Objectives and Scope of this Report

1.4.1 Objectives

The first objective of the present work is to explore the quantitative problems associated with effective EEM through a detailed literature review of the recorded problems of previous offshore monitoring efforts. The second objective is to explore the possibilities offered by a variety of quantitative methods to circumvent or alleviate the pitfalls identified in other monitoring programs. The third objective is to offer recommendations on the quantitative aspects of EEM to assist industry, Environment Canada, and other federal and provincial agencies to design an effective EEM program. Both the problems encountered by past offshore environmental effects studies and other problems anticipated on theoretical grounds are included.

1.4.2 Scope

This report deals only with the quantitative aspects of EEM; that is, with where and when to measure the monitored variables and how to analyze the resulting data. This is essentially the sixth point under the conceptual framework given in Section 1.1.2. Other than to define the quantitative problem, it does not address the associated question of what variables should be measured, nor how in practice they can be measured. With respect to EEM of offshore oil production, these latter questions have formed the subject of the extensive literature already mentioned (Section 1.1.2).

The present emphasis on the quantitative aspects of EEM is not intended in any way to depreciate the importance of other aspects of a comprehensive approach and conceptual framework to EEM. The most elegant experimental design will fail if the measured variables are irrelevant to the objectives of the program, if the logistical planning of the fieldwork is inadequate, or if institutional structures capable of responding to the results of the program do not exist. Equally, however, an EEM program that is ideally

designed in all other aspects will fail if its statistical design or other quantitative aspects are inadequate.

1.4.3 Acknowledgements

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2.0 ENVIRONMENTAL EFFECTS MONITORING OFFSHORE: RECENT EXPERIENCE

The concept of EEM is barely ten years old, whereas the working life of an offshore oil production platform is typically twice as long. In consequence, no EEM of offshore oil production could have been completed, in the sense of extending from pre-development, baseline measurements, through the construction and production phases, to the monitoring of long-term contaminants after the platform is closed down.

Perhaps more importantly not only has no such EEM program been completed, none has yet been attempted, if "EEM" is defined as in Section 1.2. Canada does not yet have any offshore oil production installations. In the United States, which has such installations in the Gulf of Mexico, off southern California and in Cook Inlet, Alaska, the only substantial environmental effects studies of offshore oil production have been made in the Gulf. The three projects undertaken there were specifically designed as short-term studies, twenty five years or more after oil production began (Carney, 1987). The other major offshore oil fields in the western world are those in the United Kingdom and Norwegian sectors of the North Sea. In those fields, no effects monitoring per se has been attempted. Instead, an on-going series of research projects has sought to answer specific questions about environmental effects, using fieldwork around whichever offshore facility is most relevant to the particular question (Dicks, 1982). The Australian offshore oil fields in the Bass Strait have had less study of environmental effects, while the developing nations have not attempted any effects monitoring.

There have been several effects studies around offshore exploration wells. Some of these have been completed, in the sense of extending through the working life of the well, since that is a brief period. Most of this work, however, has involved short-term, highly specific research (for example, NORDCO Ltd., 1983; Yunker et al. 1985). Only the Mid-Atlantic Block 684 Study and the Georges Bank Monitoring Program have approached EEM, as it is understood here. (Carney, 1987).

There are, however, several important quantitative and qualitative differences between the monitoring of exploration and production wells. These prevent a simple extrapolation of techniques from one to the other. One difference is the duration of the operation. This imposes more logistic, organizational and budgetary problems for EEM of oil production than for the monitoring of exploration activities. The long duration of production operations also introduces the possibility of chronic environmental effects and of effects centered not on the platform itself, but on areas of sedimentation which receive transported pollutants. Another difference between production and exploration lies in the spatial scales of the offshore facilities. Most EEM work around an exploration well is concentrated within one or two kilometers of the site, since little effect has been recorded at greater distances (Carney, 1987). In contrast, a production platform can be surrounded by a collection of satellite wells, storage tanks, ship moorings and the like which extend far beyond two kilometers from the central site. Clearly, EEM of such a complex necessitates more than a simple lengthening of the transects radiating from an exploration well. A final example of the differences between exploration and production is the types of pollutants generated. The

most serious pollutants from exploration rigs, barring major accidents, are probably the drill muds and drill cuttings (Dicks, 1982; Menzie, 1982). These also occur in the early phases of oil production, but are later eclipsed by a combination of produced water (formation water and injected water) and displacement water from storage facilities (Dicks, 1982; Menzie, 1982; Somerville et al., 1987). The different utilities of water and drill cuttings will demand quite different EEM designs if these are to measure the environmental effects of the water- and cuttings-borne pollutants.

The first, and one of the most important, conclusions of this review is, therefore, that the EEM proposed for the initial offshore oil development on the Grand Banks is largely novel. While past experience in many fields may aid the design of the EEM program, it must be a new design and not a small modification of a previous approach.

Since there have been no programs for EEM of offshore oil production, taking "EEM" as it is defined in Section 1.2, the present chapter cannot be a conventional review. Instead it is a broad overview of four groups of studies: (1) the three reports of environmental effects measurements around Gulf of Mexico oil production installations, (2) the environmental effects research program in the northern North Sea, (3) recent approaches to EEM off the US coasts outside the Gulf, and (4) existing Canadian plans for EEM of oil production at Hibernia and in the Beaufort Sea.

2.1 Gulf of Mexico Experience

The three major environmental effects studies that have been carried out in the Gulf of Mexico have recently been comprehensively reviewed by Carney (1987), with particular reference to their quantitative and design failings. In general, Carney's (1987) major points appear to be valid; however, he, however, was either unaware of the need for valid treatment replication (Section 5.4) or chose not to stress this. His advice should be closely examined, with reference to replication, before it is applied. Carney (1987) restricted his review to the benthic environment and that bias is retained here. LGL Ltd. et al. (1985) have also reviewed the Gulf of Mexico studies, with less of the quantitative emphasis stressed here and by Carney (1987). Their conclusions are, however, broadly the same as those of the more recent reviews.

2.1.1 Offshore Ecology Investigation

Environmental effects studies in the Gulf of Mexico began with the Offshore Ecology Investigation (OEI; field seasons 1972-4). This was intended to examine the effects of offshore oil production in the "worst case" region: the Louisiana coast. OEI used a complex array of stations, comprising several "ambient stations" in areas assumed to be exposed to the effects of petroleum production, nearby "control stations" in areas assumed not to be exposed to those effects, five offshore transects and a scatter of regional stations. Upon this station array, the various collaborators performed their own research projects with, apparently, little coordination (Carney, 1987).

Carney (1987) has criticized the Offshore Ecology Investigation for:

- 1) not gathering sufficient prior information before designing the elaborate field effort, which led to a failure to allow for natural variation in the study design,
- 2) not clearly defining the central goals and also failing to produce, a priori, operational definitions of such items of concern as "ecosystem health",
- 3) a lack of coordination between the various parts of the OEI, leading to both a failure to study some numerically-dominant organisms and an inability to relate the findings of the various parts to one another, particularly the relation of the biological results to abiotic variables including ambient hydrocarbon concentrations,
- 4) a station array in which the "controls" were merely assumed, with no empirical evidence, to be unaffected by oil production and also in which there was insufficient replicate sampling to establish anything, given the expected natural variation,
- 5) pooling samples or data, thus hiding the true variance and giving a false impression that the results were meaningful,
- 6) not using quantitative analyses other than purely descriptive methods, such as classification (cluster analysis), and
- 7) presenting opinions based on these dubious results as though they were firm conclusions.

Carney (1987) acknowledged that the overall conclusion of OEI was that natural variation had prevented a conclusive statement of the effects of oil production, but noted that OEI's results have been taken as evidence of "no effect" rather than "no proven effect" by other workers. Clearly, this is a particularly dangerous outcome for EEM which, in statistical terms, stems from β being too high (Section 5.2).

To Carney's (1987) list of criticisms of OEI, can be added:

- 8) that the irregular scatter of stations used in OEI would preclude analysis by any means, regardless of the degree of natural variation and the number of replicate samples taken at each station.

Many of these criticisms would be unfair if applied in other than a dispassionate review, since they benefit from fifteen years of hindsight. OEI provides a list of pitfalls to be avoided by careful program planning in the future.

2.1.2 Central Gulf Platform Study

OEI was succeeded by the Central Gulf Platform Study (CGPS) of 1978-9. This was largely a study of the fates and effects of chemical pollution from

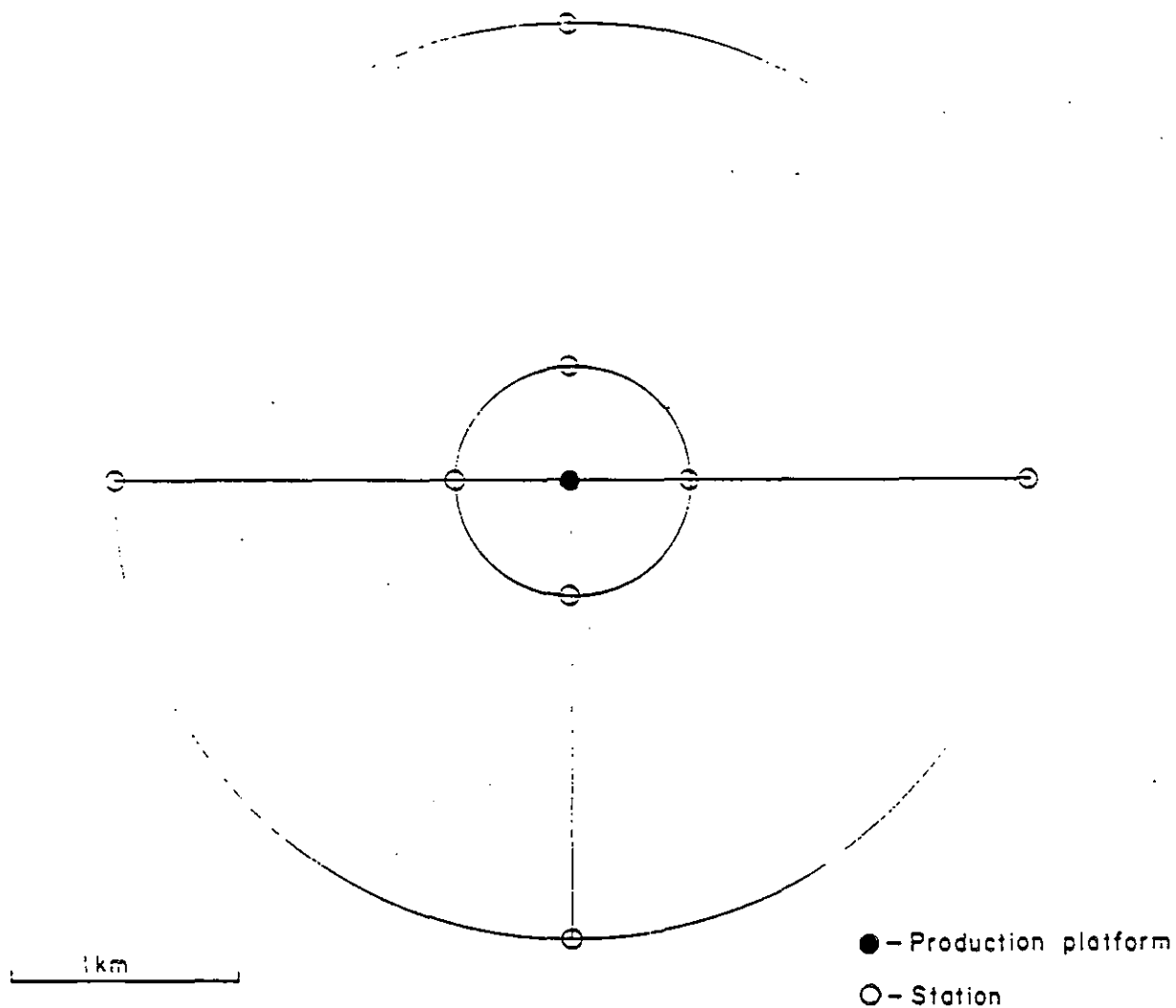
offshore production platforms, with only limited attention given to faunal analyses.

The major improvement of CGPS over OEI was in the later program's station array. This included four production platforms as "primary sites", each sampled at eight stations arranged at 500 and 2000 m from the site, on a "bullseye" pattern of four radial transects oriented on the cardinal compass points (Figure 2.1). Each primary site was paired with a "control", which was assumed to be relatively unaffected by oil production but which was explicitly not assumed to be pristine. These were, therefore, not controls in the normal sense but rather sites with a different treatment level. This is a subtle difference that is unimportant if the data are only crudely analyzed.

Carney (1987) recognized that CGPS was an improvement over OEI, but still found several points to criticize. These can be summarized as:

- 1) the goals of the CGPS were still poorly defined, with no operative definition of the "impact" that the study sought to detect and no prior hypotheses to be tested,
- 2) the station array could have been well suited to a final analysis by ANOVA, but the sample replication was discarded by pooling and the analysts proceeded with an array of less powerful technique such as subjective interpretation of data tables, classification and the correlation of biological and other environmental variables,
- 3) such inferential statistics as were used were poorly handled in several ways, with particularly severe errors in the use of correlation analysis, and
- 4) the stated conclusions were not supported by the weak analyses used, while there was a tendency to express opinion as fact.
- 5) CGPS worked stations no closer than 500 m from the monitored production platforms, whereas it is now clear that only very subtle environmental effects will occur that far away (Davies et al., 1984), so the study was always unlikely to detect anything,
- 6) most of the analytical techniques used were not suited to the goal of the study: classification is a purely descriptive process which could only detect the effects of pollution if those effects constituted large changes in the community, whereas correlations of biological to abiotic environmental variables yield no information on the effects of pollution unless the pollutant level is the environmental variable analyzed, and
- 7) the remaining analytic technique employed, subjective interpretation of the data, is a valuable first step, but because it is subjective it cannot be used to reject null hypotheses or to show causality (see Section 3.2.1).

CGPS provides a good example of a statistical pitfall that was not addressed in Chapter 5; the misapplication of classification or cluster analysis. The



CENTRAL GULF PLATFORM STUDY "BULLSEYE"
NEAR-FIELD STATION ARRAY

available classification methods are varied and flexible, allowing the skilled analyst to produce widely different results from a single data set (Clifford and Stephenson, 1975). Thus, interpretations of results from this methodology is difficult. If clusters are used as a measure of the condition of the environment, the EEM program must incorporate a test of the difference between impacted and control clusters. Most classification theory has regarded two groups. Johnson and Wichern (1988) observed:

In theory, the generalization of classification procedures [to more than two groups] is straightforward. However, not much is known about the properties of the corresponding sample classification functions and, in particular, their error rates have not been fully investigated.

The "robustness" of the two group linear classification statistics to, for instance, unequal covariances or nonnormal distributions can be studied with computer-generated sampling [Monte Carlo] experiments.

Quantitative comparisons of clusters are possible, using cophenetic correlation coefficients (Sokal and Rohlf, 1962) or the angular relationships between ultrametrics (Jardine and Sibson, 1971). These methods do not appear to have been applied to EEM and, since they can only make pair-wise comparisons of clusters, their value with such data is uncertain. In short, classification has its place in data analysis, but when used in EEM as the measure of environmental effect, it can only be expected to assist in the detection of the most obvious of ecosystem changes.

CGPS also made errors in its use of correlation coefficients, as Carney (1987) pointed out (Sections 5.2 and 5.3). A total of 12,000 such coefficients were calculated. With the critical value of α set at 0.05, there would have been 600 Type I errors, that is falsely rejected null hypotheses, if there were no true relationships between the analyzed variables. In practice, multiple inter-correlations within ecological data would be expected, such that a few underlying causes would produce several thousand "significant" correlations in the CGPS analyses. Large numbers of correlation analyses would not assist in elucidating the causes themselves.

Carney (1987) identified a third problem with the CGPS correlations: they assumed linear relationships between the biotic and the abiotic variables. This can be an acceptable assumption over small ranges of the variables, however, over wider ranges, the relationships are unlikely to be even monotonic curves. Rather, the biological variables would be expected to show an initial increase with an increase in the abiotic variable, reaching some maximum at the species' optimal environmental condition and then declining. The result would be an asymmetric, unimodal curve that is poorly represented by a correlation coefficient.

Finally, Carney (1987) criticized CGPS, much as he had OEI, for failing to formulate hypotheses to be tested and for failing to carry out the necessary

preliminary research to aid program design. These are, perhaps, unfair comments. At the time of OEI and CGPS, the Gulf of Mexico ecosystem and the effects on it of oil production were too poorly known for useful hypothesis formulation, other than as null hypotheses for statistical tests which were little used in either study. Equally, in all matters except perhaps cost, OEI and CGPS were the preliminary studies for future EEM. That the latter has not been carried out is not a reasonable criticism of the former. Nevertheless, designers of EEM programs should realize that OEI and CGPS are less than adequate models for future work in part because they lacked these design elements.

Despite all of these criticisms, CGPS had some notable design strengths, particularly the use of replicated study sites (the multiple production platforms), the inclusion of control sites (albeit imperfectly selected) and the use of crossed transects at each study site. All of these features have value.

2.1.3 Buccaneer Gas and Oil Field Study

The third major environmental effects study in the Gulf of Mexico, the Buccaneer Gas and Oil Field Study, overlapped chronologically with CGPS (survey seasons 1976-7: Carney, 1987). The Buccaneer Field, off Texas, was chosen for this work because its relative isolation should have allowed near-pristine control areas to be included in the station array. The benthos sampling effort for this study also relied on radial transects extending from the production platform. These transects were not symmetrically arranged, however, and were at least partially aligned with suspected areas of particular environmental effect. The stations on each transect were arranged either at regular intervals or with their spacing increasing with distance from the central site, apparently on the grounds that impact should be most prevalent close to the platform. Three replicate grab samples were taken at each station on each quarterly survey for a year. Unfortunately, this replication was discarded by either pooling the samples or analyzing means of raw data without reference to their variances (Carney, 1987).

The Buccaneer Study repeated several of the errors of OEI and CGPS, but Carney (1987) also identified the following problems:

- 1) the coordination of the various study components was poor; the data from each component were plotted as contour maps, and compared as the primary integration of the various data types,
- 2) such sample replication as was included in the design was discarded by pooling,
- 3) the data were analyzed with a combination of classification and ordination techniques and subjective interpretation of plots, which are subject to many criticisms discussed elsewhere in this report, and
- 4) when the Study showed a decrease in benthos abundance within 100 m of the production platform, but no significant change in diversity, the authors concluded that this was evidence for the effect of scour, not

chronic pollution. This was presumably based on the assumption that pollution, is not a "natural" stress such as scour, but will lower diversity. Their data, however, were insufficient to distinguish between these causes.

Carney (1987) also criticized the Buccaneer Study's use of the modelling approach. He approved of the use of modelling in the formulation of hypotheses and in the design of the field program. The Buccaneer Study, however, built its models after completing the fieldwork. Thus, they were the Study's conclusions, not its starting points. Carney's (1987) suggestion of a priori modelling is a sensible one, however his criticism of the Buccaneer Study stems from a decade of hindsight.

The following additional criticisms of the Buccaneer Study can be made:

- 5) the deliberate alignment of the transects imposed a subjective element on the data, removing some generality from any conclusions that might have been drawn, and
- 6) the use of asymmetric radial, rather than diametric, transects would have complicated the elimination of monotonic spatial trends from the data, had analysis proceeded that far.

OEI, CGPS and the Buccaneer Study did not detect any important effects of oil production, but it is felt that, because of design faults, they could not detect any but the most obvious of environmental damage. Conclusions of "no detectable effect" are still being cited as evidence of "no effect" (Somerville et al., 1987) and may lead to the perpetuation of errors in EEM designs, environmental management decisions, and perceptions of the environmental risk of offshore oil operations.

2.2 North Sea Experience

Offshore oil and gas production has proceeded in the North Sea for many years. Almost all of the oil fields lie in the northern part of the area, and hence in the United Kingdom and Norwegian sectors. The Norwegians have required some mandatory routine monitoring work, but this has only been compliance monitoring (Section 1.2; LGL Ltd. et al., 1985). Until very recently the British government has not imposed monitoring requirements on their oil industry (Gillam, 1987; Johnston et al., 1987). In the United Kingdom studies of the environmental effects ("biological effects" in the British terminology), of oil production, therefore, tend to be an environmental calibration of the end-of-pipe chemical compliance monitoring. In consequence, although a wide variety of pollution studies and environmental effects research projects have been carried out in the North Sea, long-term, large-scale, routine EEM programs have not been attempted. LGL Ltd. et al. (1985) have presented a detailed review of non-quantitative aspects of the North Sea environmental effects programs studies.

The primary cause of the North Sea emphasis on effects studies, rather than effects monitoring per se, was the realization that biological effects monitoring of offshore oil installations is simply not practical at present

(Dr. A.D. McIntyre, pers. comm.). The need for effects monitoring was acknowledged as early as 1975 by the ICES Working Group on Pollution Baseline and Monitoring Studies in the Oslo Commission and ICNAF Areas (McIntyre et al., 1978). This led to a major report on the feasibility of such monitoring (McIntyre et al., 1978) and, in 1979, to a workshop at which the existing knowledge base was reviewed (McIntyre and Pearce, 1980). The latter produced a realistic appraisal of the problems facing effects monitoring programs (Gray et al., 1980) and precipitated the research which McIntyre et al. (1978) had called for as a prelude to EEM. That research has been ongoing, but it has not led to the rapid introduction of effects monitoring programs (McIntyre, 1984). Indeed, there has recently been a subtle, but highly meaningful, change in terminology from "biological effects monitoring" to "biological effects studies" (Dr. A.D. McIntyre, pers. comm.).

Given the research emphasis of effects studies in the North Sea, the large body of practical experience acquired there is of only indirect relevance to EEM. Furthermore, the bulk of published methodological material resulting from all of this research has concentrated on what to measure, rather than the quantitative questions of where, when and how often to measure it or of how to analyze the data, which are of relevance here. McIntyre and Pearce's (1980) workshop volume, for instance, contained only one paper (Jones, 1980) that addressed experimental design, amidst thirty five other research contributions. Jones' (1980) paper concerned only the inshore environment.

All of this research has, of course, produced many reports on the environmental effects of oil (Davies et al., 1981, 1984; Clark, 1982; Dicks, 1982; Hartley, 1982; Somerville et al., 1987). Material on the quantitative approaches taken in the North Sea must, therefore, be extracted from papers whose prime purpose is not to elaborate on methodology. The remainder of this section is, therefore, an overview of the quantitative aspects of some EEM-like approaches that have been used in the North Sea. Since the work there has comprised a series of relatively discrete research projects, a wide variety of methods have been used and this overview is not exhaustive, but it is selective of some of the more relevant references.

The process of effective research planning is apparent in North Sea work, leading to explicit choices of such factors as the best environment to study, for example, benthos for point source pollution but plankton for ocean-wide studies (Gray, 1980; McIntyre, 1984), the best species to study given that those of economic importance usually cannot be monitored efficiently (Dicks, 1982), and an optimal meshing of the ecotoxicological approach with mesocosm studies and community-level field measurements (Davies et al., 1981). As emphasised above, such clear planning with explicit objectives is essential to efficient research and monitoring. Its regular use is a notable feature of the North Sea work.

The careful planning does not, however, extend into the quantitative aspects of the projects. There is an explicit recognition that biological effects should be most readily detectable on pollution gradients (Gray, 1980; Gray et al., 1980; Dicks, 1982; Hartley, 1982). This leads naturally to transect arrays of stations and, for offshore point sources of pollution, to multiple transects radiating from the site (Davies et al., 1981, 1984; Hartley, 1982).

None of these authors, however, appear to have considered the principal limitation of transect designs, that is, that they lend themselves only to correlation and regression analyses which cannot determine causal relationships (Section 5.3). While these seem to be the most frequently used station arrays, Hartley (1982) expressed a preference for regular grids, presumably centered on the point source.

Several other quantitative aspects of project planning are mentioned in the North Sea literature, but do not seem to be rigorously applied. Inferential statistics are notably lacking in this literature. Hartley (1982) raised the question of optimal sample size, but then approved of using a single sample per station, which would prevent almost any statistical analysis of the resulting data. Gray (1980) and Gray et al. (1980) correctly recommended stratified random sampling for increased efficiency, but this idea does not seem to have been implemented. The advantage of including control sites in the design has been raised (Gray et al., 1980) but most workers have preferred to assume that the ends of their transects represent "background" conditions. They have not tested this assumption.

Some other aspects of planning that have been recommended in the North Sea literature are the need for long-term studies if chronic effects are to be detected (Gray, 1980) and the concentration of sampling into a fixed time each year such as April to June in the North Sea, to avoid seasonal cycles in the benthos (Hartley, 1982). Both ideas have some merit, though the latter is discussed at more length elsewhere in this report (Section 5.5). Finally, Gray et al. (1980) noted the importance of Type II errors in the analysis of monitoring data; a major theme of the present work.

There are often few quantitative analyses of data. Gray et al. (1980) did suggest the use of analysis of variance, ordination, reciprocal averaging and numerical classification techniques. Several North Sea scientists have suggested, or used, relatively advanced mathematics when calculating indices of environmental effect (Gray and Misra, 1979). When they reach the crucial analytical step of relating that effect to the presence of, or distance from, an oil production platform, however, North Sea projects tend to fall back on subjective interpretations of graphical displays of their data (Davies et al. 1981, 1984; Somerville et al., 1987).

As stated elsewhere in this report, careful, educated interpretation of graphs must form part of any analysis of EEM data. Used alone, however, graphical interpretation can suggest hypotheses but not test them (Tukey, 1977). The absence of quantitative analysis of the data is seen as a failing of the North Sea work. It prevents much of the experience gained in that region from being of relevance to the quantitative design of Canadian EEM programs.

If the analytical phase of the North Sea effects research is flawed, at least the work is carried to a final conclusion. Much of it appears as papers in primary scientific journals where it is available for worldwide study and critique. While we recognize that this sort of final conclusion and publication is much easier with, and more appropriate to, effects research than EEM, it is one of the relative strengths of the North Sea work.

All of these points are well illustrated by Davies et al.'s (1984) study of the effects of oil-based drill muds. This was a review of existing, but mostly unpublished, data sets. Hence the authors did not have control over the sampling design. They examined the data in the form of scatter plots of several variables (oil concentration, naphthalene concentration, numbers of benthic species, diversity, abundance of some species) against distance from the relevant production platform. From the horizontal right-hand limb of these scatter plots, they concluded:

"In the surveys studied, the more remote stations could be considered non-contaminated and thus act as reference sites."
(Davies et al., 1984, p. 366).

Their plots do appear, to show approximately exponential decline in pollution and its effects out to perhaps 3000 m from the platforms, with constant levels beyond that distance. Davies et al. (1984) did not, however, attempt to fit exponential curves, nor to determine the distance at which their data deviated from the curve into a constant, background value. They also did not consider the possibility of two pollutant dispersal mechanisms, one operating perhaps by sediment transport and thus causing a marked gradient in pollution near the production platform while the other, perhaps involving water column transport, rapidly disperses the contaminants over a much larger area. This could happen, for example, in using water-based drilling muds. Their conclusion:

"Generally speaking, background levels [of hydrocarbons] are reached within 3000 m" (Davies et al., 1984, p. 367).

is therefore questionable.

In effects research, it may be acceptable to overlook the effects of widespread, and hence dilute, pollution, while studying the more obvious effects of the intense near-platform contamination. This is not, however, acceptable in EEM precisely because it is the task of EEM to determine whether the effects of the widespread pollution are negligible.

In terms of environmental effects, Davies et al. (1984) studied only the macrobenthos. Once again, this is acceptable if the research question is phrased as "the effect of oil based muds on the macrobenthos." It is not suitable if the aim is to monitor the effects of oil based muds on the environment.

In summary, the North Sea work on the environmental effects of oil production has been structured as a series of partially well-planned research projects, rather than as EEM per se. In the process, it has built the only substantial body of knowledge concerning the effects of offshore oil production. The studies have not, however, used quantitative analytical methods and, in particular, have not used inferential statistics. Their conclusions are, therefore, somewhat subjective and remain untested by more rigorous techniques. The frequent conclusion that offshore oil installations cause only local effects on the environment, for instance, should not be accepted unequivocally on the basis of any of the non-quantitative North Sea work.

There are two major lessons for quantitative design of Canadian EEM programs. First, the North Sea scientists have shown that environmental effects studies can be efficiently designed and executed. More importantly, they have shown that effective EEM must be preceded by extensive supporting research. In the opinion of these researchers to date, ten years of research in the North Sea have still proven insufficient. While Canada might hope to build on the North Sea results, their experience suggests that EEM for offshore Newfoundland cannot be both fully operational and fully efficient without the necessary preparatory work.

More specifically, the North Sea work has raised three useful suggestions: that long-term studies are essential (Gray, 1980), that stratified random sampling could lead to greater efficiency (Gray, 1980; Gray et al. 1980) and that grid sampling may be preferable to radial transects for sampling around offshore platforms (Hartley, 1982).

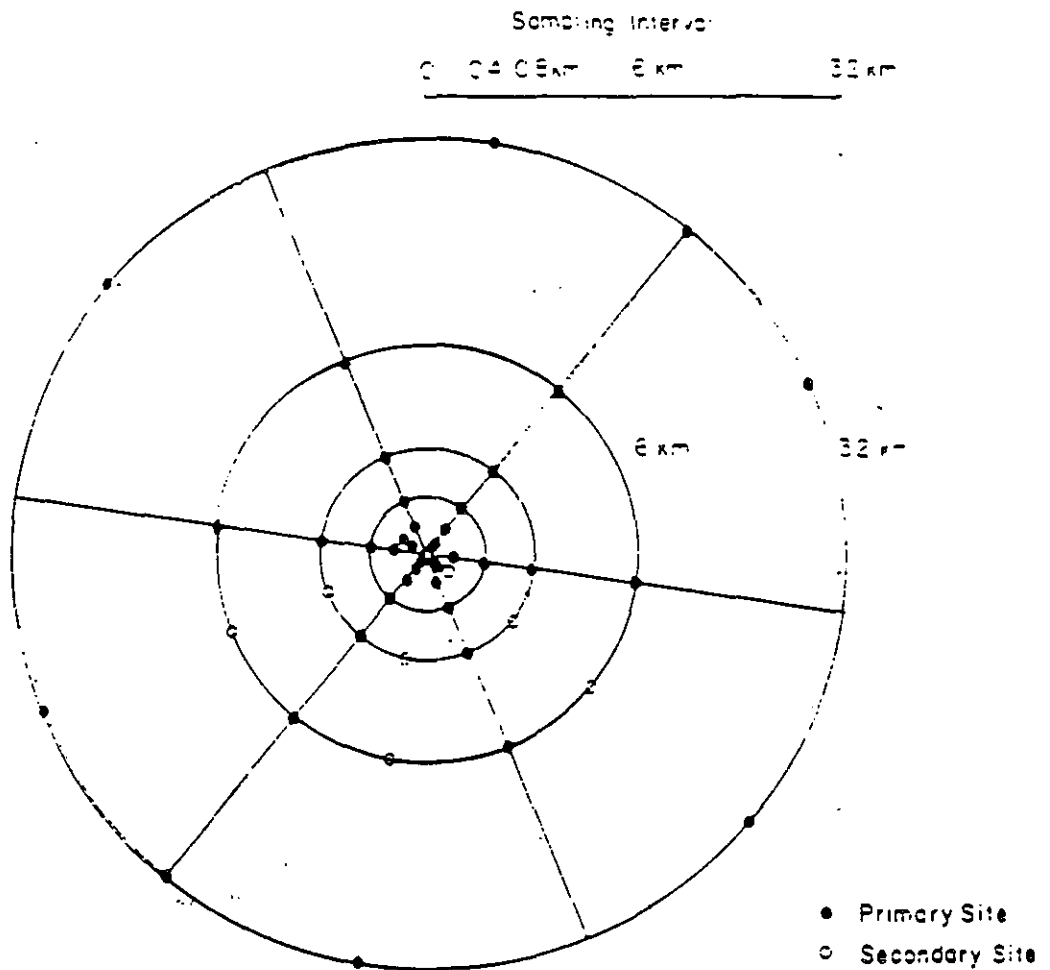
2.3 Other Monitoring Programs off the Coasts of the United States

Three recent offshore EEM Programs have built upon the experience gained in the Gulf of Mexico and the North Sea. These are the Mid-Atlantic Block 684 Study, the only comprehensive program for EEM of an offshore exploration well, the Georges Bank Monitoring Program, a first attempt at regional-scale integrated monitoring of both exploration and production, and the California Monitoring Program which is intended to be an integrated study of multiple production platforms. These three programs represent the nearest approaches to date to the sort of offshore EEM intended for Hibernia.

2.3.1 Mid-Atlantic Block 684 Study

This study of an exploration well off Atlantic City, New Jersey was partly coincident in time with the Gulf of Mexico studies discussed in Section 2.1 (field seasons 1978-9). Compared to those Gulf studies, the Block 684 work was well planned. In particular, it had excellent coordination between its various parts and it incorporated an explicit definition of the "impacts" sought (Carney, 1987).

Nevertheless, this study was still flawed. The fundamental problem lay with the design of the station array (Figure 2.2). This was an elegant refinement of the "bullseye" pattern used in the Central Gulf Platform Study (Section 2.1). At Block 684, six symmetrically-arranged radial transects were laid out. These thus formed three diameters, which would simplify the elimination of monotonic spatial trends at the analysis stage, though the pattern seems (Carney, 1987) to have been chosen rather because regularity aids mapping. Stations were placed on each transect at logarithmic intervals: 50, 100, 200, 400, 800, 1600 and 3200 m from the platform. This should have led to 42 stations being included in the array, but some of the 3200 m stations were removed while additional radial part-transects were added between some of the main transects. The final design included 48 stations, of which only 40 or 41 were both sampled and included in the analyses. This arrangement was apparently based on a model used in microbial bioassay that essentially assumes that any effects of oil exploration will spread diffusively from the platform (Carney, 1987).



Source: Carney (1967)

MID-ATLANTIC BLOCK 684 STUDY: NEAR-FIELD STATION ARRAY

Since this design was, in effect, the ultimate development of the "golfing green" approach, its limitations must be discussed in detail. If the marine environment were as homogeneous as a green, if the relevant pollutants did diffuse outward from the platform and if those pollutants had a linear dose/response relationship, there might be a logarithmic decline in environmental effects with distance from the platform. In such a case, and if the objective of monitoring was to describe the parameters of the decline with distance, a station array involving logarithmic station intervals on radial transects would be optimal.

The marine environment is, however, spatially heterogeneous (Section 5.5). The quantitative effect from a given level of pollutant will, therefore, depend on what kind of patch the station is set in. In addition, most pollutants will be spread by advective rather than diffusive processes. The patchiness of advection, caused by water column gyres and similar features, will lead to a patchiness in the pollutant loading to the already patchy natural environment. Furthermore, the response of the environment to pollutants is not linear; environmental effects will often appear when the pollutant loading exceeds some critical value. As Carney (1987) has argued, this combination of factors will not result in the logarithmic decay of intensity of effect with distance from the platform that was assumed by the designers of the Block 684 Study. Rather they will lead to the seabed having "affected patches" interspersed with "unaffected patches". The proportion of the seabed lying within an affected patch may decline logarithmically with distance, but the intensity of effect within each patch is quasi-independent of its location. With such a distribution of effects, it is virtually certain that every station within 100 m of a platform will fall on an affected patch, and hence multiple stations within this range are largely unnecessary. 3200 m from that platform, however, few stations would be expected to lie on affected patches. Hence, estimation of the proportion of the seabed affected will require a relatively high station density at such distances. The principal transects of the Block 684 Study design, however, placed twelve stations within 100 m of the platform (circa one per 2500 m²) but only six within the 100 m band centered 3200 m from the platform (circa one per 300,000 m²). Clearly, this design is suboptimal.

This is not, however, the only flaw of the Block 684 Study station array. One of the conditions for that array to be optimal is that its objective is to describe the variation of the effect with distance from the platform. While this is an interesting research question, and may indeed have been the objective at Block 684, it is an inappropriate focus for most EEM. As is repeatedly argued in this report, EEM requires not description but decision-oriented inference. For "discordant" effects, that inference requires an hypothesis test. Often the null hypothesis for such a test will be of the general form:

H₀: The level of variable x at y metres from the platform
does not exceed 110% of its background value, as measured
at a control site.

although clearly an actual hypothesis must be much more specific. This form arises because it is known in advance that the area directly under the platform will be disrupted. Useful impact predictions must therefore take the form of limits to the expected spread of this disruption, and EEM should focus on the testing of those predictions.

The Block 684 Study array fails with such a null hypothesis for two main reasons. First, it makes no provision for testing a prediction based on a distance, y , other than one of the pre-selected logarithmic intervals. Second, it provides no reliable measure of the background level of the variable. Where such station arrays have been analyzed, there seems to have been an assumption that the lack of a trend between the outer two or three stations on a transect is sufficient indication that they are beyond the influence of the platform and hence represent a valid background control.

The danger that an entire EEM field program could be completed before analysis showed a significant trend between the outer pair of stations (and hence no valid control) is a concern. The end-of-transect-control idea also requires that the program be able to demonstrate the absence of a trend in effects between the outer stations. The best that can be achieved is the demonstration that there is no significant trend between these stations. Furthermore, the background condition is not homogeneous but rather spatially patchy (Section 5.5).

Furthermore, as the transect is extended to remove its end from the zone of influence of the platform, so its end moves into natural spatial patches less and less like those near the platform. Thus, the measured "background" conditions become irrelevant to the hypothesis test.

In practice, the interpretation of the Block 684 Study data included some comparisons with more distant Bureau of Land Management Benchmark Program stations (Neff, 1987). If used as controls, these stations would clearly avoid most of the problems of assuming that the ends of the transect represent background conditions. In view of the spatial patchiness typical of marine environments (Section 5.5), however, it is not surprising that these comparisons with one or two control stations gave confusing results; near-site, post drilling data were more similar to the BLM station A3 data than to the near-site, pre-drilling ones (Neff, 1987), preventing clear conclusions from being made of the effects of drilling.

The station array was sampled three times: before drilling, immediately after drilling and one year later. Two replicate samples were taken at each station on each sampling period. This before/after/later design clearly has some merit for exploration, though not production, platforms. The sample replication is also a marked improvement over some of the earlier work in the Gulf of Mexico (Section 2.1). Nevertheless, sample replication cannot provide valid treatment replication (Mar et al., 1985; Millard et al., 1985; Section 5.4). Given the high natural variability described in Section 5.5, no design compromised in this way can hope to reveal very much.

Some of these problems may have been realized by the scientists involved. At least, they appear to have designed their program in such a way as to lead to

a final, major ANOVA, but they abandoned this intention and resorted to less demanding analyses (Carney, 1987). These analyses included classification, Principal Components Analysis (PCA) and some simple Two-way ANOVA's. PCA provides fewer alternative methods than does classification, and so is more objective. There is no guarantee, however, that the narrower range of options is any closer to the appropriate approach than any selected classification technique would be. When used in EEM, PCA is subject to most of the other faults of classification techniques. It is, however, possible to compare the results of PCA's by Procrustes Rotation (Digby and Kempton, 1987), which yields more useful comparisons than the cophenetic correlation or ultrametric analyses that must be used with clusters (Section 2.1.2).

The intended use of ANOVA in the Block 684 Study at least promised the possibility of an hypothesis test, but the inappropriate data provided by the station array were partially pooled. Any remaining validity was thereby lost and the resulting acceptance of the null hypotheses demonstrated the inadequacies of the Study, rather than the lack of an effect of the platform (Carney, 1987).

Finally, the Block 684 Study used analyses only of total faunal density, diversity, evenness and species number per unit area (Carney, 1987). None of these were likely to change under any but the most obvious of environmental damage. Hence, even if the quantitative aspects of this Study had been well designed, it would have been unlikely to detect less extreme but still serious environmental effects.

This criticism of the Mid-Atlantic Block 684 Study is lengthy, and there is no doubt that the Study was flawed. In relative terms, however, it was much better designed and executed than any of the contemporary Gulf of Mexico studies (Section 2.1). In part, it is this better design that permits detailed criticism, which is simply not possible for the Gulf work.

2.3.2 Georges Bank Monitoring Program

The next major environmental effects study, in US waters, of an offshore oil installation was the on-going Georges Bank Monitoring Program. This grew by a prolonged and complex development, which in turn resulted from the equally prolonged political arguments concerning drilling on Georges Bank (Hughes and Van Dusen, 1987; Leschine and Lahey, 1987). The active field phase of the Program began in 1981, but was largely cut short by the withdrawal of all exploration platforms from the Bank in August 1982 (Danenburger, 1987). There has been no drilling there since, because of a combination of public reaction, low world oil prices and the fact that the eight wells drilled in 1981-2 were all "dry". In consequence, the Georges Bank Monitoring Program has effectively come to a halt.

This is particularly unfortunate, from the perspective of EEM methodological development, because this Program (and the Environmental Studies Program for Georges Bank of which it forms a part: Leschine and Lahey, 1987) was the first attempt at an on-going environmental monitoring and management process for the offshore oil industry. The Program explicitly combined a long-term regional study with short term, site-specific work around a selected exploration well

(Carney, 1987; Leschine and Lahey, 1987). Had drilling continued, other wells might have been monitored, depending on the results obtained at the first one (Leschine and Lahey, 1987). Thus, something of the iterative, study-specific approach used in effects research in the North Sea was to be applied to the exploration phase. This was sensible, since exploration wells are themselves short-term phenomena. The published plans for monitoring on Georges Bank have not addressed the more complex question of long-term site-specific monitoring of production platforms. At present, it seems unlikely that there will be such platforms on Georges Bank in the foreseeable future. Certainly, this type of broad-scale planning of EEM promises to incorporate regional questions and the needs of long-term monitoring of oil production from the inception of the exploration phase. It also gives the maximum lead time for the EEM program, which can only aid monitoring efficiency.

The Georges Bank Monitoring Program, at the time of its intense field phase of 1981-2, had four main objectives:

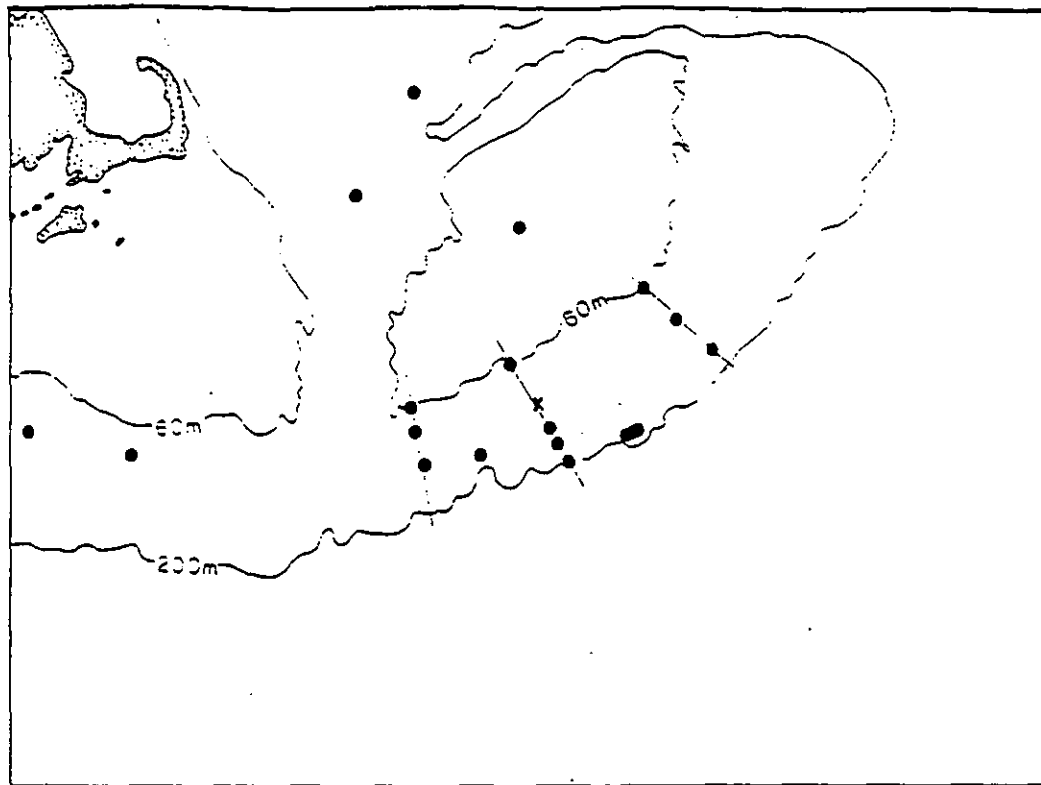
- 1) to monitor materials discharged during drilling;
- 2) to examine the areas in which discharged materials accumulate, and to measure the concentrations of those materials;
- 3) to measure the background levels of contaminants, and
- 4) to measure changes in benthic populations and to examine their possible relations with drilling discharge pollution (Leschine and Lahey, 1987).

The first objective is most appropriately characterized as Compliance Monitoring. Objective 2 was a valuable idea for EEM of oil production that may have been unnecessary for exploration wells.

There are only two apparent problems with these objectives. Carney (1987) has emphasized the lack of a clear definition of the "impacts" sought and has stressed the danger of such a deficiency. The only effects studied, and thus the only ones that can be detected, are in the sediment/benthos system. There are, apparently, plans for adding water column studies in the future (Leschine and Lahey, 1987). In view of the cessation of drilling in 1982, it is unfortunate that these studies were not incorporated into the earlier design.

The regional station array (Figure 2.3) comprised three transects, one through the drilling area and one each upstream and downstream with one station per transect at each of three depths, together with ten other stations in suspected accumulation areas. Some of these stations continued earlier ones worked by the Bureau of Land Management as part of their abortive Benchmark Studies Program of 1977 (Carney, 1987; Leschine and Lahey, 1987).

The site-specific work centered on a well drilled for Mobil Oil in Block 312. A total of 29 stations were arranged on four primary and four secondary radial transects, arranged at 45° intervals around the platform. Stations were set on the primary transects at 200, 500, 1000, 2000, 4000 and (in one case) 6000 m from the platform, and at a subset of these distances on the secondary transects (Figure 2.4). The primary transects were aligned to the axes of the

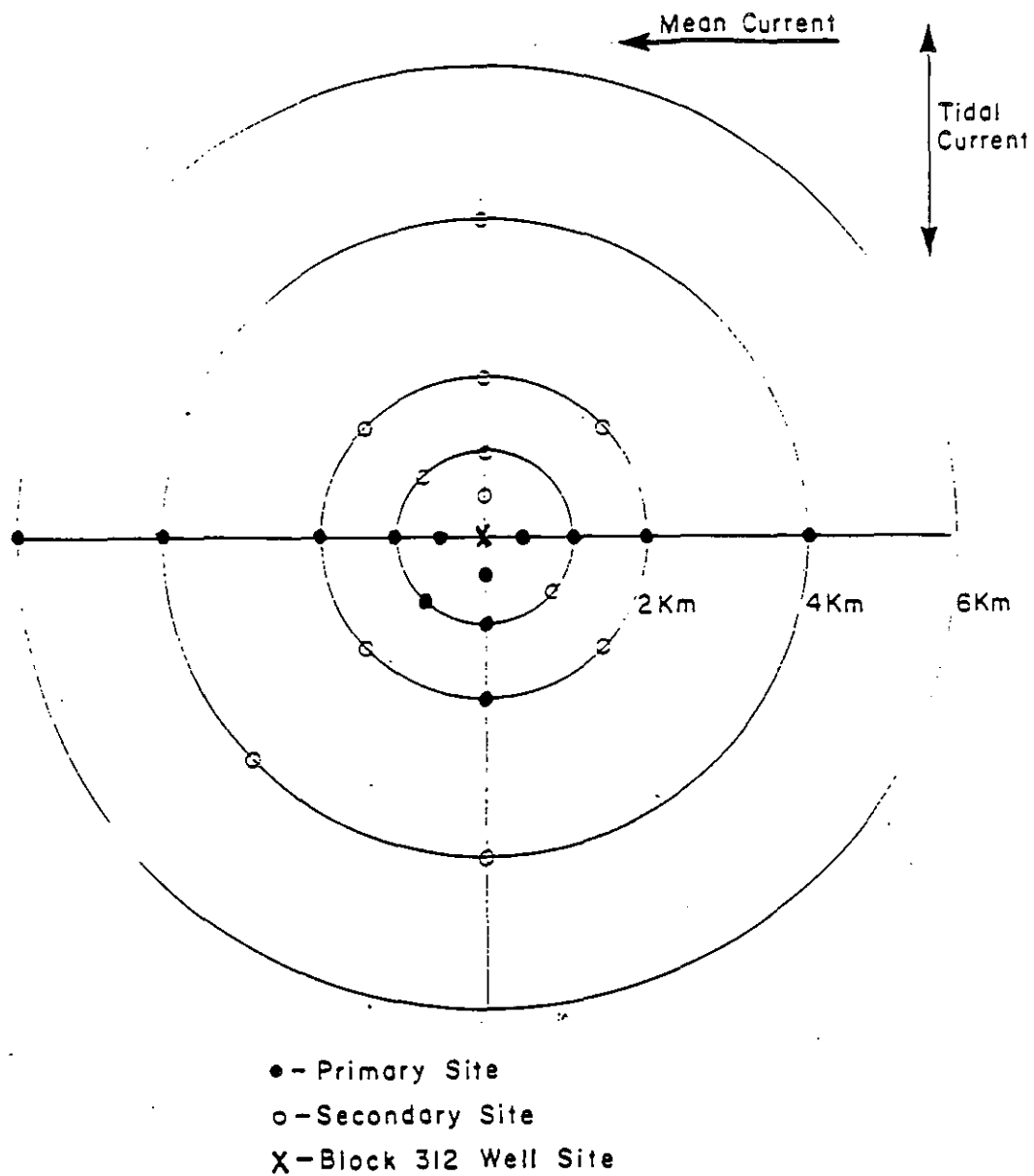


● - Primary Site

x - Block 312 Well Site

Source: Carney (1987)

GEORGES BANK MONITORING PROGRAM REGIONAL STATION ARRAY



Source: Carney (1987)

GEORGES BANK MONITORING PROGRAM NEAR-FIELD
STATION ARRAY AROUND THE EXPLORATION WELL
SITE IN BLOCK 312

major tidal stream and the mean current vector, which are orthogonal at this site (Carney, 1987).

Both the regional and the site-specific stations were sampled quarterly. In the case of the Block 312 work, this gave two surveys before drilling commenced, two during drilling (December 1981 to June 1982: Danenberger, 1987) and four thereafter. Six replicate samples were taken at each station with each type of grab used (Carney, 1987). These were supplemented with bottom photography though it is not yet clear whether the photographs can be analyzed quantitatively. Data were collected on hydrocarbon concentrations, sediment types and the benthos (Leschine and Lahey, 1987).

Carney (1987) approved of this design, in marked contrast to his comments on earlier work. He particularly approved of: the excellent coordination between the various facets of the Program, the addition of long term regional studies to the design and the explicit use of replicate sampling to estimate variances rather than to gain "representative" means. He expressed the hope that the final analyses, which are still awaited, would incorporate ANOVA and hence provide explicit results. There should be an important caveat. Variances estimated by replicate sampling are sampling variances; they are not the same as the variances estimated from valid treatment replication. The use of sampling variances in ANOVA as though they were treatment variances is pseudoreplication (Hurlbert, 1984) and will produce serious errors (Section 5.4). The Georges Bank Monitoring Program design provides no form of treatment replication and so valid ANOVA would be difficult, if not impossible.

Indeed, the Program continues other flaws from earlier work, despite its several improvements. The concentration on the sediment/benthos system has already been mentioned. The site-specific station array is little different to that used at Mid-Atlantic Block 684 and is essentially subject to all of the criticisms in Section 2.3.1. Many of the other comments made about the Offshore Ecology Investigations, Central Gulf Platform Study and Buccaneer Gas and Oil Field Study (Section 2.1) could still be applied to the Georges Bank Monitoring Program, albeit the errors are less glaring than in the earlier work. In short, the Georges Bank work was a great improvement over its precursors, but it still had flaws.

2.3.3 California Monitoring Program

A proposal to drill production wells off the central Californian coast led to the final offshore EEM program yet to be put into operation: the California Monitoring Program (CAMP: Brewer *et al.*, 1987). This work is a derivative of the Georges Bank program in its conceptual framework and design, though it did not commence until after the exploration drilling had been completed in its area. CAMP has been planned as three phases, with an intended duration of ten years. Phase I (1984-6) was designed as the preliminary to EEM, with the general objectives of describing background conditions and providing the necessary information for quantitative experimental design. Phase II (1986-91) should span the initial production drilling and lead to Phase III once EEM has gathered sufficient information to permit improvements in the design. Drilling at Platform Hidalgo was scheduled to commence in 1987, with Platform

Julius to follow in 1989 and others thereafter (Brewer et al., 1987). At present, further production drilling off California appears to have ceased.

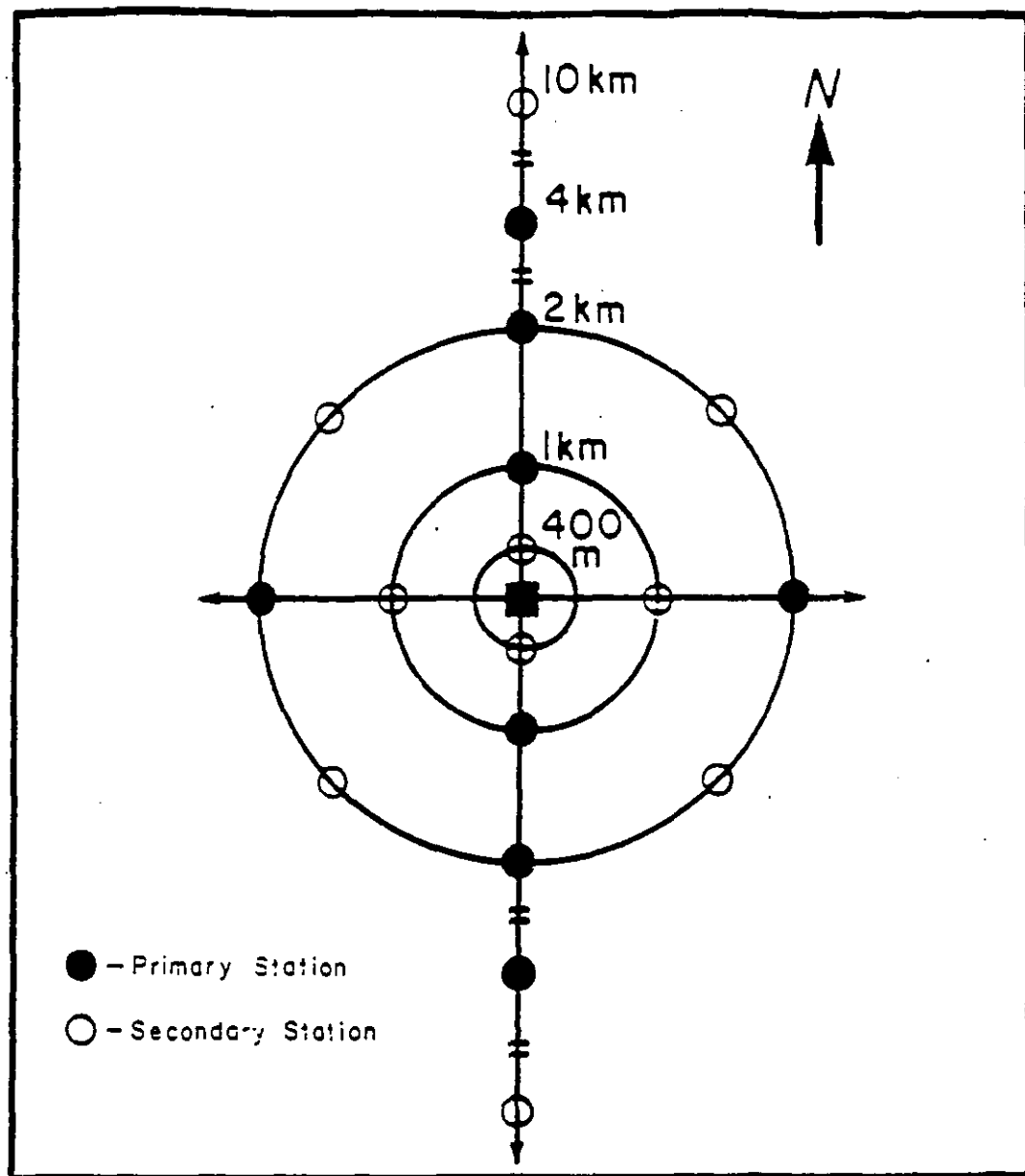
Phase II of CAMP centers on the detection of regional-scale effects and specifically on relatively-distant, subtle changes. The major environmental degradation close to a production platform is, therefore, accepted as inevitable and ignored for EEM. In consequence, the primary survey stations of the near-field array around the Platform Julius site are placed 1000, 2000 and 4000 m from the production platform. These are arranged on a single diametric transect, aligned north-south, and supplemented with other primary stations 2000 m due east and west of the platform. There are also secondary stations at 400, 1000, 2000 and 10,000 m from the platform, for a complete near-field array of 18 stations (Brewer et al., 1987: Figure 2.5). The Platform Hidalgo site is close to areas of rocky bottom. Current plans for the near-field monitoring of this area are essentially qualitative. They involve photographic inspection of selected rocky features (Brewer et al., 1987). No suggestions of analytical methods suited to such EEM data have yet been published. Finally, the program design includes nine regional stations; three on each of three inshore-offshore transects. One of these stations has been deliberately placed in an area of known sedimentary deposition (Figure 2.6). Every station is to be worked once each fall (Brewer et al., 1987).

The affinities of this design with that of the Georges Bank Monitoring Program are obvious. CAMP is, therefore, subject to most of the praise and criticisms expressed in Section 2.3.2. Where the two programs differ, it is mostly because CAMP uses a reduced design: nine regional stations against nineteen, eighteen near field stations on two main radii against 29 stations on four main radii, annual surveys against quarterly. In the absence of evidence that even the Georges Bank Monitoring Program was sufficiently powerful to yield useful results, this further reduction for CAMP is unfortunate. With only three replicate samples per station per year (Brewer et al., 1987), CAMP is very unlikely to detect the subtle effects that are its declared objectives.

CAMP has not yet proceeded to the analytical phase. There is, however, a declared intention to follow Green's (1979) so-called "optimal design" (Brewer et al., 1987). It is not at all clear how this could be done with the CAMP station array, since Green (1979) assumed only one control and one impacted station. This uncertainty is, however, immaterial since the inadequacies of Green's (1979) design are already well known (Skalski and McKenzie, 1982; Hurlbert, 1984; Stewart-Oaten et al., 1986; Section 5.4).

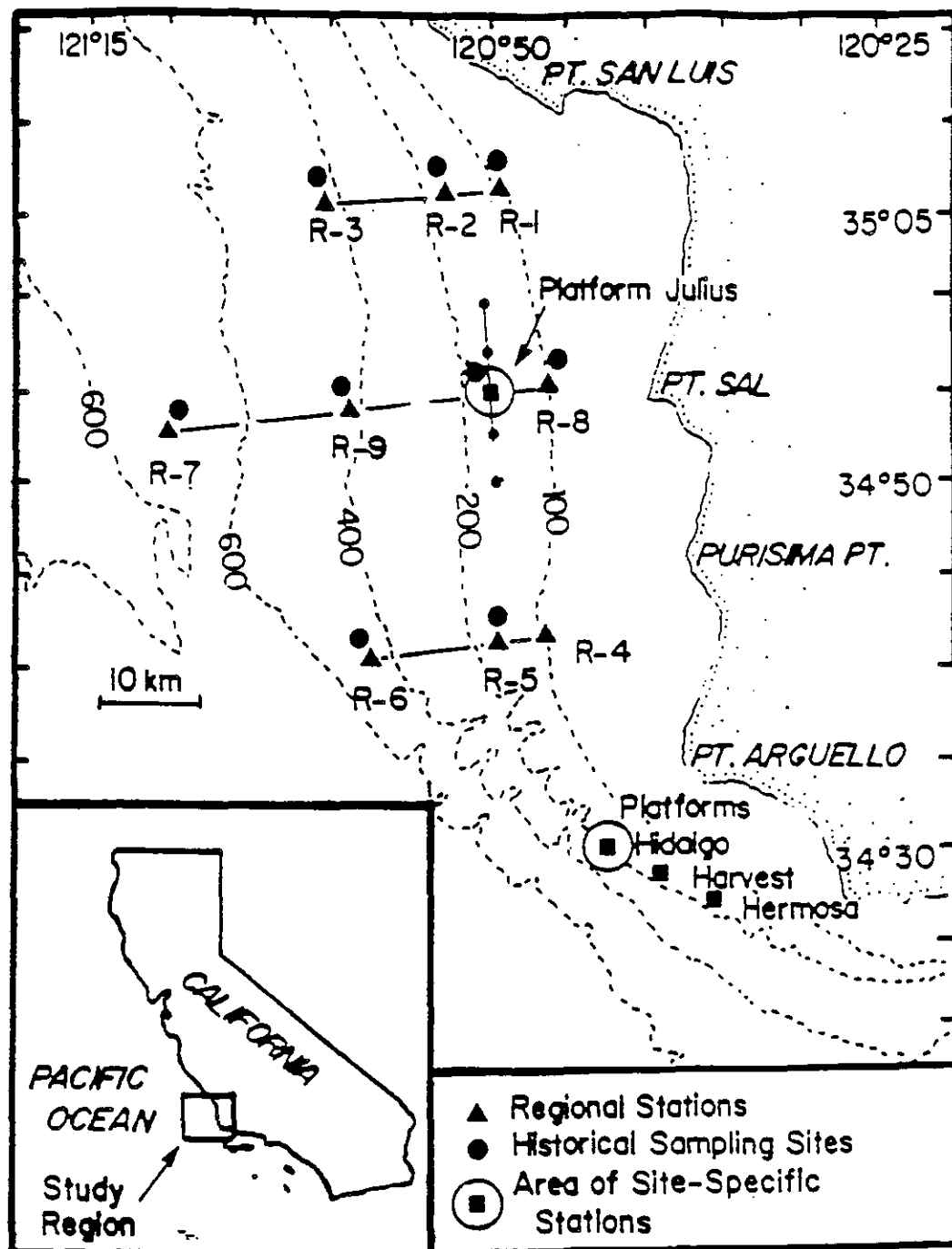
2.4 Canadian Plans for EEM of Offshore Oil Production

As there were until very recently no firm plans for the immediate development of an offshore oil field in Canadian waters, so there are no definite plans for EEM of such a development. There have, however, been a few detailed proposals of which three are reviewed here: (1) a proposed strategy for EEM on east coast fields (LGL Ltd. et al., 1985), (2) a draft monitoring plan for Hibernia, prepared for Mobil Oil Canada Ltd. (Shawmont Martec Ltd. and E.G. & G. Environmental Consultants, 1983) and (3) the extensive Beaufort Environmental Monitoring Project (BEMP: Indian and Northern Affairs Canada, 1985; Indian and Northern Affairs Canada and Environment Canada, 1985; Everitt



Source: Brewer et al. (1967).

CALIFORNIA MONITORING PROGRAM (CAMP)
NEAR FIELD STATION ARRAY
AROUND PLATFORM JULIUS



Source: Brewer et al. (1987)

CALIFORNIA MONITORING PROGRAM (CAMP) REGIONAL STATION ARRAY

et al., 1985). These three encompass the current state of Canadian offshore EEM planning.

2.4.1 EEM Strategy for Canada's East Coast

LGL Ltd. et al. (1985) reviewed past EEM programs, much as the present chapter does but without the emphasis on quantitative aspects. Those authors went on to propose a strategy for offshore EEM in eastern Canadian waters south of 60°N, with specific monitoring programs for the Venture gas field and the Hibernia oil field. Their comments on past experiences have been incorporated into the earlier discussions (Sections 2.1 and 2.2) and are not repeated here.

The proposed offshore EEM strategy bears little relationship to previous experience, whether in the North Sea, Gulf of Mexico or elsewhere. This is appropriate, since EEM as presently required in Canada is so novel. LGL Ltd. et al. (1985) made three major suggestions:

- 1) the need for careful planning, both a priori and ongoing, together with careful integration of all parts of the program;
- 2) the need for detailed quality control procedures throughout the program;
- 3) the need for pilot studies, particularly to permit efficient statistical design.

LGL Ltd. et al. (1985) also confine their EEM strategy to the seabed environment. They provide several points to support this choice:

- 1) it is easiest to define and measure effects in this environment (p. 37);
- 2) the sediments are an accumulating medium with relatively low variability that serves to integrate contaminant effects, whereas the water column is a dispersing medium with high variability (p. 39);
- 3) contaminant concentrations tend to be higher in the sediments, making detection easier (p. 39);
- 4) some benthic fauna filter large quantities of water while remaining in one location, and can thus integrate the cumulative stress at one spatial point (p. 42);
- 5) sediment and benthic samples are easily replicated (pp. 39, -2);
- 6) "Through the use of sediment cores, historical data on the input of contaminants can be obtained for use as a reference without the need for pre-development baseline studies" (p. 39);
- 7) monitoring in the Gulf of Mexico and North Sea has been unsuccessful because the results were uninterpretable, primarily because of insufficient information on natural variability in the water column (p. 40); and

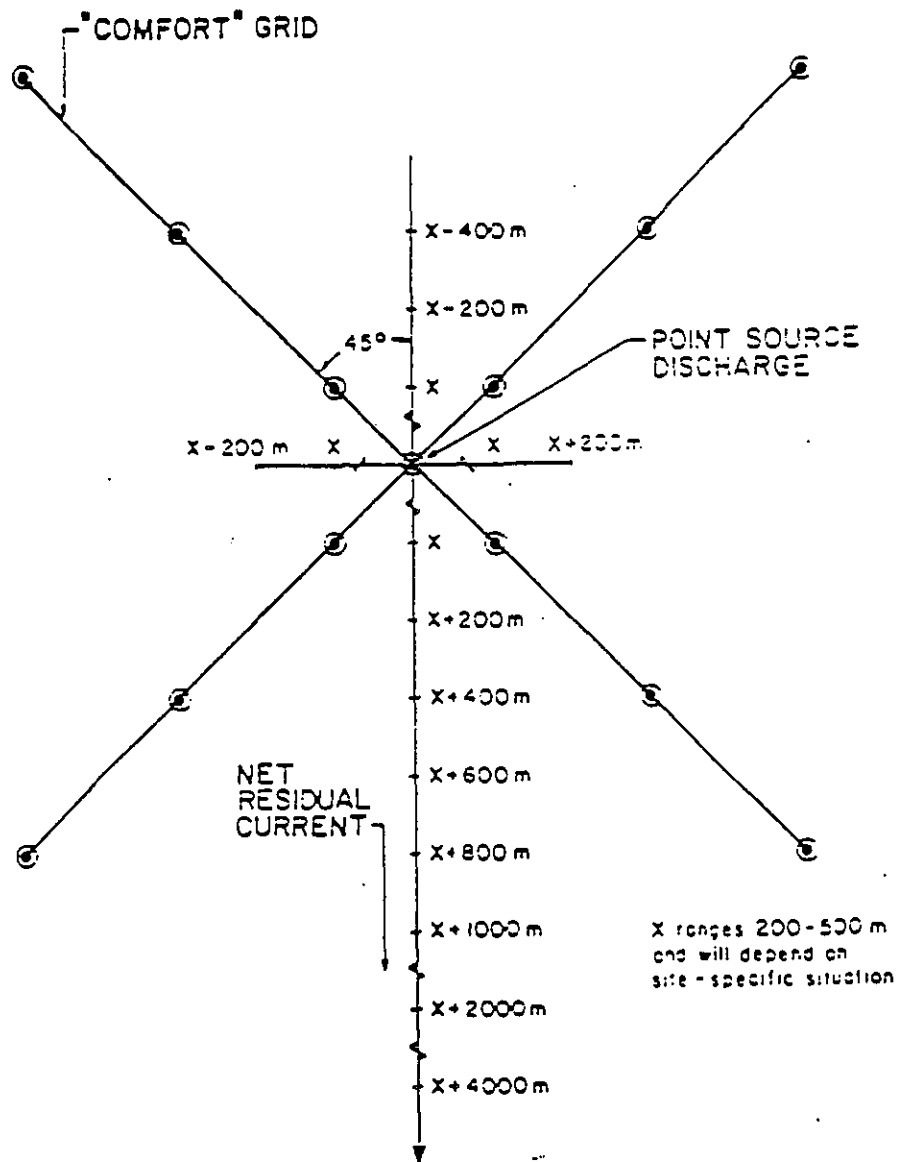
- 8) contaminant concentrations in the water column are several orders of magnitude below the 96 h LC_{50} values for most marine organisms, except very close to the outfall pipe, and so only very subtle effects can occur in the water column (p. 40).

All of these points are debatable. For example, the claim that pre-development studies are unnecessary because sediment cores preserve an historical record is erroneous. The coarse gravel sediments near Hibernia will preserve no such record. Even if a core could be extracted from them, movements of pore waters will have disrupted and confounded the annually deposited contaminants. In the finer sediments, bioturbation may disrupt the upper layers as to prevent the reading of any record for the critical few years before development. Moreover, in the absence of pre-development studies, any sedimentary record will be uncalibrated relative to present conditions.

LGL Ltd. et al. (1985) recommended a modified form of the "bullseye" station array (Figure 2.7). This included eight radial transects, at 45° intervals. Only the one transect aligned down the residual current had many stations, however. The others had only two or three stations per transect and the station spacings differed on various transects. None of these unique features have any advantages over the Mid-Atlantic Block 684 Study or Georges Bank Monitoring Program designs, nor did LGL Ltd. et al. (1985) suggest any such advantages. In keeping with their suggestion to ignore the "high impact" zone (item 6 above), their innermost stations are 200 or 500 m from the platform (CAMP's 400 or 1000 m; see Section 2.3.3).

They further suggest (p. 41) that the survey vessel be navigated with particular accuracy and that it remain on station for the duration of sampling. If a transect station array is followed, this demand has some merit. Clearly, if the theoretical station positions are 50 or 100 m apart, navigation or ship handling errors of as little as 10 m could have a serious effect on the analyses. If, however, the intention is to minimize the influence of spatial variations on the time series of data from each station, this concern with accuracy is not appropriate. Sufficient precision to ensure that minimization simply cannot be obtained offshore.

Given a gravity based-production platform as a stable support for the necessary antennas, it is technically possible to measure the position of a ship anywhere in the near-field with a precision of a few metres, using surveying equipment. The navigational uncertainty cannot, however, be improved beyond this range in practice. Given this degree of knowledge of position, the survey vessel's captain must still place his ship at the required point and hold it there. Offshore, in a tideway and often with a heavy beam sea, this is simply impossible for a conventional vessel. Given a dynamic positioning system, as on some oil rig supply vessels, station keeping to 10 m precision may be possible. This must be the precision of the position of the sampling winch's meter wheel, however, not of the vessel's navigational antennas, giving the captain an extra problem. Even given suitable vessel positioning and a point sampling device (a grab, for example, but not a dredge nor a plankton net), the device will be dropped on 100 m of wire to the



STATION ARRAY RECOMMENDED
BY LGL LTD ET AL. (1985)

seabed. A 6° deviation of that wire from the vertical, caused by water movements, will displace the sampler 10 m horizontally.

The errors in navigation, station keeping and in the fall of the sampler are cumulative, so that only "lucky" samples taken by the most careful personnel can come within some 10 m of the nominal station position. Under realistic conditions, much larger errors can be expected. Yet, as outlined in Section 5.5, spatial variation at much finer scales than this can be anticipated. Thus, spatial variation will be complexed with temporal changes in a time series of data, no matter how much care is devoted to navigational precision. The minimum achievable precision is close to the maximum acceptable when the station spacing is only 100 m, and thus LGL Ltd. et al. (1985) were correct to demand great attention to that precision. It is insufficient to remove spatial variation from the data, however, so the EEM design must incorporate that variation in an analyzable manner no matter how carefully the survey vessel is navigated.

Given their sampling array, LGL Ltd. et al. (1985, p. 40) suggested sampling annually, because they believed measurable effects are not likely to accumulate in less than one year. More frequent sampling has never been advocated, to our knowledge, not because the effects will not accumulate quickly but for two quite different reasons: (1) so that sufficient samples can be collected, and (2) so that the complications of inter-annual variations in the seasonal cycle can be minimized in the data. It is very unlikely that β can be sufficiently low in EEM work if only one sample is gathered per year.

The final important quantitative aspect of LGL Ltd. et al.'s (1985, p. -3) design is their explicit intention to use only analyses of contaminant and effect gradients. They suggest that this avoids the need to establish natural variability, and hence reduces the required sample sizes. This is incorrect. Confining analyses to gradients removes the effects of medium-term temporal variations, that is, those that would occur between surveys, but has no other variance-reducing effect. Even this slight advantage is gained only by restricting the analyses to some form of correlation or regression, though not necessarily to the simple linear forms. These are the only obvious ways by which gradients can be analyzed. As outlined in Section 5.3, such analyses cannot demonstrate the causality that LGL Ltd. et al. (1985) require in their definition of EEM. Correlation and regression analysis should be regarded as estimating "associations". If, as LGL Ltd. et al. (1985) propose, only spatial gradients at points in time are analyzed, EEM would even lose the ability to detect changes in those spatial gradients coincident in time with the construction of the platform. Thus, EEM would become no more than a matter of mapping effects around the platform.

Environmental management decisions will only be based on such maps if the effects are grossly obvious and have an unavoidably-obvious cause related to the platform, for example smothering of the benthos by drill cuttings immediately under the discharge pipe. If an EEM program that relied on comparisons of maps did detect any more subtle changes, it would be unable to establish that they were caused by the oil development. The environmental managers would not then be likely to act on the information provided. In short, EEM based exclusively on studies of gradients would rarely be able to

influence environmental management and, as such, would be a waste of resources.

On the basis of their proposed strategy, LGL Ltd. et al. (1985) further proposed EEM programs for oil production at Hibernia and for gas production at the Venture field. In their Hibernia program proposal, they further restricted EEM so as to include only three monitoring projects:

- 1) to detect tainting in exploited marine species,
- 2) to detect elevated hydrocarbon and heavy metal levels in sediments, and
- 3) to detect changes in sediment particle sizes and the benthic community structure.

For the first of these they recommended only "opportunistic" sampling (p. 62) which can never be more than semi-quantitative. They did suggest that the taste panel studies, designed to detect tainting, be supplemented by assays of Mixed Function Oxidase (MFO: Payne, 1974, 1985) to determine whether the fish have been exposed to hydrocarbons.

The second and third monitoring projects approximated to an application of the Sediment Quality Triad approach (Long and Chapman, 1985; Chapman, 1986). That application, however, was compromised by the lack of any prior definition of the concentrations of hydrocarbons or heavy metals that would be judged to represent "contamination" (Carney, 1987). More seriously, LGL Ltd. et al. (1985) did not suggest what measures of "community structure" should be employed, nor the degree of change in those measures that should be regarded as an "effect". Because of these deficiencies, hypotheses such as:

"Offshore production facilities will discharge sufficient quantities of wastes to alter benthic sediment chemistry and substrate and habitat characteristics near the discharge site, and these factors will cause an alteration in benthic community structure" (LGL Ltd. et al., 1985, p. 69)

are untestable. They can give no basis for the calculation of β and hence no guide to the optimum sample sizes that, as LGL Ltd. et al. (1985) acknowledged (p. 72), must be estimated.

The hypothesis is also untestable by field sampling alone because of its stress on "cause". Benthos surveys could, at most, address the hypothesis:

H_0 : Benthic community abundance x = from the production platform site will not change, relative to size control.

LGL Ltd. et al.'s (1985) proposal limits EEM to only two ultimate effects: tainting of fish consumed by humans and changes in benthic community structure. No water column monitoring may be an acceptable acceptable policy decision. There is, however, no scientific reason to suppose that benthic changes are the only environmental effects of offshore oil production.

2.4.2 A Proposed EEM Program for Hibernia

Shawmont Martec Ltd. and E.G. & G. Environmental Consultants (1983) (SMEGG) prepared a surveillance and monitoring plan for Hibernia on behalf of Mobil Oil Canada Ltd. This plan was not developed beyond the draft stage but as it incorporates the only specific Hibernia EEM program proposal, it merits attention here.

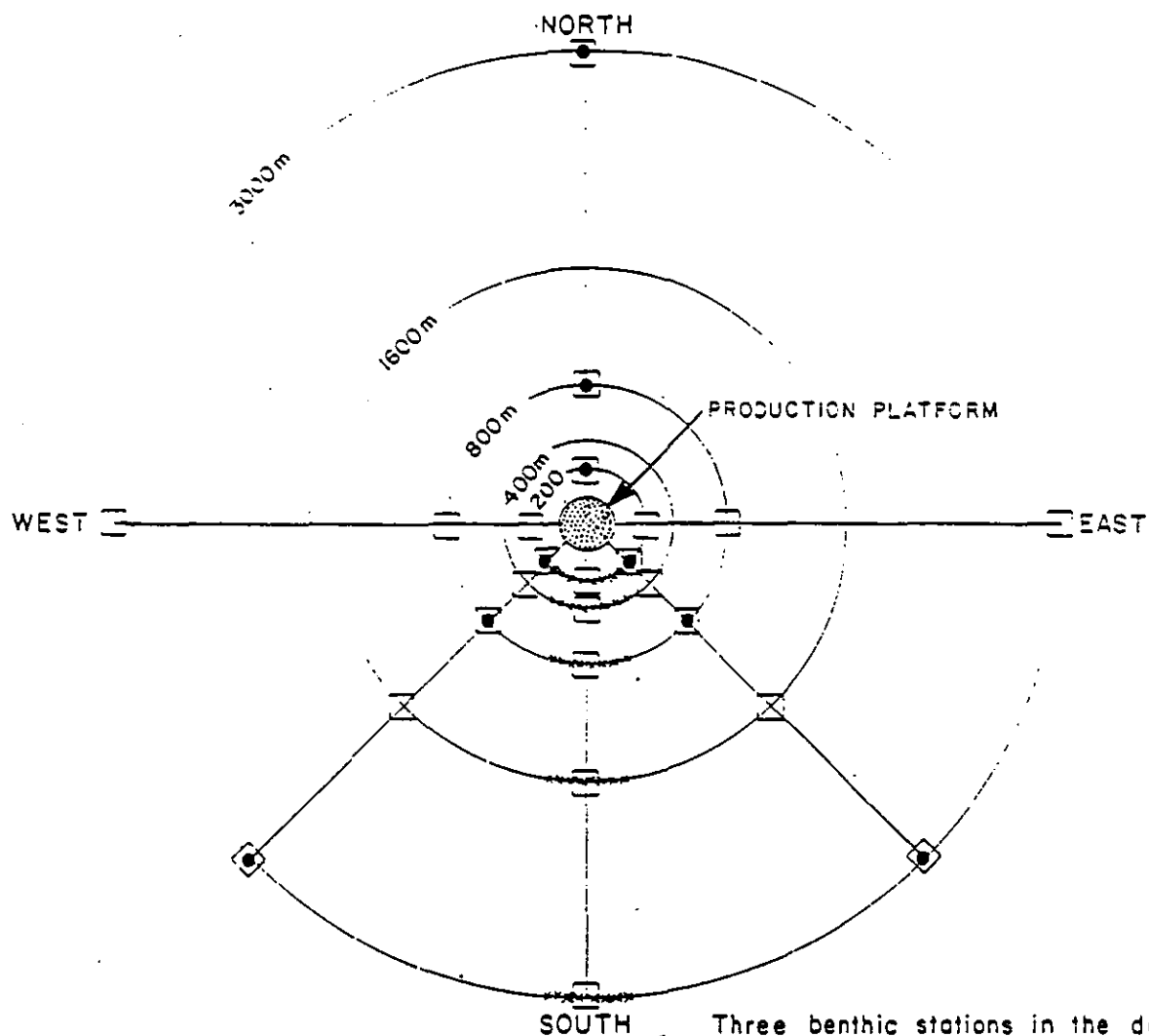
The proposed station array was essentially a derivative of that used in the Georges Bank Monitoring Program (Section 2.3.2). It incorporated both a near-field array and three distant stations in the Carson Canyon, an assumed depositional area. The near-field array included six radial transects (oriented towards north, east, southeast, south, southwest and west respectively) with up to five stations per transect (at 200, 400, 800, 1600 and 3000 m from the platform: Figure 2.8). Not all stations were to be worked for all component studies. The ichthyoplankton tows and bottom trawls for fish, for example, were to be confined to the five stations on the southern transect, while only nine of the thirty possible stations were to be sampled for pleoclastic bacteria (SMEGG, 1983). This station array is subject to many of the same criticisms as are the arrays employed in the Mid-Atlantic Block 664 Study, the Georges Bank Monitoring Program and the California Monitoring Program (Section 2.3). The fewer stations suggested for Hibernia and their southward bias would have been additional disadvantages.

SMEGG placed the same stress on annual surveys and precise navigation as did LGL Ltd. *et al.* (1985) and their plan is, therefore, subject to some of the criticisms given in Section 2.4.1. The former's EEM program was, however, more wide ranging and more detailed than the latter's, and included: "Scope for Growth" measurements, the presence of contaminants in the macrobenthos, Gray and Misra's (1979) log normal index of environmental stress and MFO induction in ichthyoplankton. SMEGG's proposed program was therefore more capable of detecting effects than was LGL Ltd. *et al.*'s (1985) later design.

SMEGG's plan included one improvement over most of those reviewed in this section; they attempted to estimate the sample size required to obtain a given probability of detecting a change. They used an expression from Green (1979), however,

$$x \pm t_{1-(1/2)\alpha} \frac{s}{\sqrt{n}}$$

that calculates the sample size, n , needed to achieve confidence limits of a chosen magnitude (for example, 25% of the mean) with a chosen probability ($1-\alpha$). It would have been more appropriate to calculate the sample size required for ANOVA to detect a chosen difference (for example, 25% of the mean) between impacted and control sites when α is set at 0.05 and β at perhaps 0.1 (Section 5.2: methods for these calculations are explained in standard statistical texts). Nevertheless, the explicit attempt to optimize sample size was a valuable advance in program planning. The required numbers of samples, approximately 20 to 200 for 95% confidence limits of 25% of the mean, are an



LEGEND

- - Benthic sampling
- - Microbiological sampling
- xxx - Ichthyoplankton tows and bottom trawls for plaice and sandlance.

Three benthic stations in the depositional environment of the middle slope near Carson Canyon $\approx$ 100 Km away, depth 700-1200 m.

STATION ARRAY PROPOSED FOR MONITORING AT HIBERNIA
BY SHAWMONT MARTEC LTD. AND E.G. & G. ENVIRONMENTAL
CONSULTANTS (1983)

indication of the scale of field effort required for even such coarse environmental measurements offshore.

A final point of concern is SMEGG (1983) treatment of "testable" and "null" hypotheses. They list eleven questions with accompanying hypotheses, such as:

"Does the elevated hydrocarbon concentration in produced water cause an increase in the density of oleoclastic bacteria, relative to the total heterotroph population?"

Testable hypothesis - Oleoclastic bacteria will increase in density, relative to the total heterotroph population, in the water column and in the sediments near the production platform.

Null hypothesis - Oleoclastic bacteria will show no response to production platform effluent." (SMEGG, 1983, p 5.36).

The associated hypotheses display errors of formulation. If the above question is indeed important, its answer would demand a test of whether, in an appropriately designed and executed experiment, the ratio:

Density of Oleoclastic Bacteria: Density of Total Heterotroph Population

increases in the presence of produced water, relative to its value in some valid control (spatial or temporal). The statistical test by which this experiment was analyzed would require a null hypothesis far more detailed than that given above, perhaps of the form:

H₀: The ratio of densities, in surficial sediments, of oleoclastic bacteria to the total bacterial heterotroph population does not increase at stations 500 metres from a produced water outfall that releases $10^4 \text{ m}^3 \text{ day}^{-1}$ of Hibernia produced water, relative to a valid control.

This null hypothesis could be tested if valid randomized replication of controls and outfalls were possible. The earlier hypothesis can not be since it includes no definition of "response" nor of the effluent loading at the measurement sites and does not bound the population of possible measurement points. In addition, the "testable" hypothesis has been inappropriately modified into the null hypothesis. The converse of "will increase" should be "will not increase", rather than "will show no response". This has two practical results. First, a test of "will not increase" is a one tailed test, rather than the two-tailed one needed to test "will show no response". Second, rejection of "will show no response" is insufficient reason to accept "will increase", whereas rejection of "will not increase" would be sufficient (Section 5.2).

The lack in the null hypothesis of "relative to the total heterotroph population", "in the water column and in the sediments" and "near the

production platform" will disrupt inference from these hypotheses in essentially the same way.

Another issue revealed by these questions and hypotheses is some confusion among "falsifiable", "testable" and "null" hypotheses, which may stem from a misunderstanding of the "scientific method". As outlined in Section 1.3, Beanlands and Duinker (1983) suggested that all environmental impact predictions should be structured as falsifiable hypotheses, following a limited interpretation of the Popperian view of the scientific method.

SMEGG sought to answer their "questions" with testable hypotheses which appear to have been intended as falsifiable hypotheses in the Popperian sense. They then inverted these into null hypotheses suitable for statistical testing. As outlined in Section 5.2, however, these tests test the null hypothesis. They give no direct indication of the probability of an alternate hypothesis. Thus, SMEGG's testable hypotheses could not be tested by their proposed EEM.

More generally, it is important for planners of EEM programs to be clear which hypothesis they are seeking to disprove and how their program can do so through logical inference. It might be desirable, for instance, for environmental effects research to test an appropriate biological null hypothesis, formulated using Occam's Razor, such as:

H₀: The density of clams 100 m from a production platform does not differ from that at a control site,

whereas verification EEM must test an impact prediction such as:

H₀: Clams will not survive within 100 m of the drill cuttings outfall on the production platform.

The latter is not literally a "null" and would not normally be an appropriate starting point for research. More importantly, the amounts of data needed to test these hypotheses are quite different. Thus, it is essential that EEM program planners explicitly, and correctly, formulate the appropriate null hypotheses for their programs at an early stage.

2.4.3 Beaufort Environmental Monitoring Program

The final major Canadian offshore EEM planning exercise to date has been the Beaufort Environmental Monitoring Program (BEMP). BEMP is fundamentally different from all of the other programs reviewed here, and it has been strongly influenced by Beanlands and Duinker's (1983) ideas. Indeed, it has yet to become a monitoring program. Like CAMP, BEMP was initiated to monitor oil production operations, after many years of exploration drilling (Everitt et al., 1985). Oil production has yet to proceed in the Beaufort. Thus, although BEMP has been active since 1983, it has not yet had anything to monitor. It has been, therefore, no more than a sophisticated monitoring program planning exercise or perhaps rather a research coordination exercise. Two principal reports on this work are by Indian and Northern Affairs Canada (1985) and Indian and Northern Affairs Canada and Environment Canada (1985).

BEMP grew out of the need, perceived by Indian and Northern Affairs Canada and Environment Canada in the early 1980's, for conceptually-sound monitoring in the Beaufort area. An early decision was taken to base the program planning on Holling's (1978) technique of Adaptive Environmental Assessment and Management (AEAM: Everitt et al., 1985) based on published studies, and personal communication with AEAM team members this approach, however, has worked better for problems of resource management than for prediction and detection of environmental impacts. AEAM was highly regarded at the time of BEMP's inception and was widely used by agencies of the US federal government. It has since fallen into disuse, for reasons too complex to be reviewed here. BEMP has been largely shaped by its use of this now-discarded approach. Many of its features could, therefore, be criticized but much of this would amount to no more than a redundant criticism of AEAM using BEMP as an example. Rather than make such criticisms, this section is largely confined to a brief description of the work done by BEMP and an explanation of its irrelevance for EEM at the initial offshore oil development on the Grand Banks.

BEMP began with a workshop in May, 1983 in which a team of facilitators and a group of scientific experts worked together to produce a simulation model of the Beaufort Sea ecosystem and the effects on it of oil production operations. The model was quickly discarded, and contrary to the standard AEAM approach was not used to generate hypotheses. Impact predictions were developed using an ad hoc application in the form of hypotheses, each with specific anthropogenic causes acting on specific Valued Ecosystem Components (VEC's) via specified "linkages" (Everitt et al., 1985; Indian and Northern Affairs Canada, 1985). These hypotheses were refined at a second workshop late in 1983 at which nineteen hypotheses were reviewed and refined. Some were discarded as being highly unlikely or impractical to test. For the others, the research required before monitoring could commence was specified.

The nature of these impact hypotheses can be illustrated by an example:

"Hypothesis 13: Shorebases and shallow water production facilities will release hydrocarbons and heavy metals at sufficient levels such that fish harvest will be reduced through tainting and heavy metal accumulations." (Indian and Northern Affairs Canada, 1985).

Each half of this hypothesis (A: tainting, B: heavy metal accumulations) was supported by a flow diagram and a five-point linkage, which served only to state the obvious relationships between oil industry-generated pollution and the contamination of fishery resources. These were supplemented by extensive and well referenced discussions. The Workshop concluded that hydrocarbon tainting of fish was possible in sheltered bays but rejected both tainting in other environments and heavy metal accumulation anywhere as being unlikely. It suggested that no research was needed to support this hypothesis and that monitoring could be by time-series measurements of hydrocarbons in fish from Tuktoyaktuk Harbour. If tainting were found, taste tests and caged fish experiments might be needed to identify the areas affected and the source of the contamination (Indian and Northern Affairs Canada, 1985).

Unfortunately, this hypothesis is untestable. The lack of a quantitative value for the amount of tainting hypothesized to occur means that the hypothesis could be true if there was only very minor, infrequent tainting. Thus, if major tainting is found, the hypothesis could be accepted. If such tainting were not seen, the hypothesis could not be falsified, and falsifiability is an essential feature of scientifically-testable hypotheses. Furthermore, the hypothesis does not state that fish will be tainted but that the fish harvest will be reduced because of tainting. This can only be tested by monitoring harvest levels and, perhaps using market survey techniques, the factors controlling them. Measurements of actual tainting may be necessary to separate real tainting from misguided public opinion but they are secondary. Thus, the proposed monitoring is largely irrelevant to the hypothesis. Even with a broad monitoring program, it seems unlikely that a decrease in catches in Tuktoyaktuk Harbour could be attributed to hydrocarbon tainting resulting from shorebases and shallow water production facilities with sufficient certainty to precipitate expensive remedial actions. For the reasons given in Chapter 5, such causal attribution certainly cannot be established "scientifically". For these reasons too, the hypothesis is untestable in an EEM context.

Following the first two Workshops, BEMP proceeded to support the proposed research projects. A third Workshop was then held in February, 1985, at which the hypotheses and their linkages were reviewed in the light of the new research results. For Hypothesis 13, the same arguments seem to have been repeated as had been put forward a year earlier. The sub-sections of the hypothesis rejected at that previous meeting were discussed once again. Indeed, the only notable addition was a suggestion to monitor Mixed Function Oxidase (MFO) as an indicator of fish exposure to hydrocarbons (Indian and Northern Affairs Canada and Environment Canada, 1985). This addition was probably irrelevant to an hypothesis concerned with harvest reductions caused by tainting; a process in which fish exposure plays only a very preliminary role. Once again, Hypothesis 13 does not seem to be an extreme example of BEMP's work and the third Workshop showed little general advance over the second.

Since the third Workshop, BEMP's research projects have continued, along with on-going reviews of the hypotheses. There does not seem, however, to have been any progress towards the development of specific monitoring program plans that relate in any way to the others reviewed in this section.

BEMP's approach to monitoring, which underlies all of this hypothesis formulation and re-evaluation, is perhaps best encapsulated in their initial definitions:

"Monitoring was defined as the repetitive measurement of variables to detect changes directly or indirectly attributable to a specific developmental activity. The primary purpose of monitoring is to determine causal relationships between development activities and environmental effects. ... In the context of BEMP, monitoring is not surveillance or a part of the regulatory process

used to ensure that industry meets the environmental terms and conditions of its operating permits. Monitoring is a scientific process, designed to test specific hypotheses linking sources of environmental impact (actions) to their expression in the biophysical environment.

Research is a test of a system process hypothesis, or baseline measurements. ..." (Indian and Northern Affairs Canada, 1985, p xxi).

These definitions seriously restrict the scope of scientific research. More importantly, they restrict "monitoring" to what is termed "experimental EEM" in the present report. In any case, only two features of BEMP might usefully be considered when planning EEM for offshore oil development.

- 1) when selecting what to monitor rather than where or how often some structured, interdisciplinary discussions are essential, and
- 2) as seen in the North Sea (Section 2.2), considerable amounts of research have been required in the Beaufort area before fully efficient monitoring can be undertaken.

2.5 Some Lessons Learned About Quantitative Design in Offshore EEM

After some 20 years of offshore environmental effects studies, the body of accumulated experience illustrates many pitfalls to be avoided in the future. There are several specific lessons however, that serve as positive guidance for the future:

- 1) careful quantitative program design is essential (LGL Ltd. et al., 1985; Section 2.4.1; Carney, 1987),
- 2) that design step must build on the results of pilot studies (LGL Ltd. et al., 1985; Section 2.4.1),
- 3) a major body of knowledge is required, based on scientific research, before monitoring can be fully effective (North Sea work and BEMP, Sections 2.2 and 2.4.3)
- 4) monitoring designs should be based on regional plans (Georges Bank Monitoring Program and CAMP, Sections 2.3.2 and 2.3.3),
- 5) depositional areas should be monitored, in addition to the near-field and general region (Georges Bank Monitoring Program and CAMP, Sections 2.3.2 and 2.3.3)
- 6) designs can incorporate replicate production platforms and multiple control sites (Central Gulf Platform Study, Section 2.1.2), though the mere existence of replicates does not constitute valid replication.

- 7) station arrays based on crossed transects are potentially useful, especially when developed to the level of the Georges Bank Monitoring Program arrays (Section 2.3.2) but Hartley's (1982) suggestion of grid arrays may be preferable.
- 8) stratified random designs could increase efficiency (Gray, 1980; Gray et al., 1980; Section 2.2).
- 9) thresholds for management action, agreed a priori by all parties, would greatly ease the monitoring task (LGL Ltd. et al., 1985, Section 2.4.1).

These points do not lead directly to a new, valid EEM design. This is to be expected, since quantitative offshore EEM is such a novel procedure, at least if it is to follow the definition used herein. Many of the problems and objections raised in this report have only become relevant because the definition and objectives of EEM have changed, particularly since Beanlands and Duinker (1983) suggested that EEM could be used in an experimental context. It is the demand that EEM establish causality in "scientifically rigorous" and "statistically sound" ways (Conover, 1985) that would render EEM impossible. Sound statistics used rigorously cannot establish causality from EEM data. The solution is to change the definition and expectations of EEM, as much as the methods used.

3.0 POSSIBILITIES AND RECOMMENDATIONS

In previous chapters of this report, current and recent offshore EEM programs have been reviewed. It is clear that a program for EEM for east coast offshore oil developments based on any of these existing designs would be unable to fulfill its objectives, as those are defined in Chapter 1. A range of approaches that, collectively, may permit the future effective design of such EEM in regard to its quantitative aspects is provided below.

3.1 Environmental Management Possibilities

3.1.1 Recognition of Problem

The first step to solving the problems confronting offshore EEM is undoubtedly their recognition. As is stressed throughout this report, EEM of offshore oil production is relatively novel. A solid conceptual base cognizant of all potential pitfalls must be established for long-term cost-effectiveness for environmental protection during offshore oil and gas development in Canada.

3.1.2 Inapplicability of Experimental EEM

The type of experimental EEM advocated by Beanlands and Duinker (1983), which forms such a key part of present monitoring objectives (Conover, 1985), is not possible offshore. The necessary statistical inferences cannot be validly drawn in the absence of treatment replication. Beanlands and Duinker's (1983) interpretation of the Popperian "Scientific Method" is not suitable to the high variances typical of environmental data. Thus, present monitoring policies should change. If that change is made appropriately, the task facing EEM program designers could be greatly eased.

The place of hypothesis-testing, experimental EEM in current Canadian environmental management practice has been outlined in Section 1.3. The extreme view of Beanlands and Duinker (1983), that environmental impact assessment should be structured as a strict hypothesis-falsifying experiment, has also been described.

The Beanlands and Duinker (1983) approach appears attractive. The scientific understanding of the behaviour of perturbed ecosystems would be greatly advanced by properly designed holistic experiments, if such could be conducted. Unfortunately, the disadvantages of the approach, from an environmental manager's perspective, are equally obvious.³ Beanlands and Duinker (1983) would only accept an impact prediction in an EIS if that prediction were structured as a testable hypothesis. Yet, the formulation of testable hypotheses is a difficult process (Sections 2.4.2 and 2.4.3). Furthermore, many socially important, highly probable and intuitively predictable, impacts cannot be tested, for practical reasons such as changes in mean year-class strength of a fishery resource, which can only be measured

³ See comments on the relationship between research and environmental management by Hecky et al. (1984), Munro et al. (1986) and others.

once per year. While experimental tests of hypothetical primary impacts such as increases in ambient levels of discharged chemicals might appear possible, tests of most impacts that would impinge directly on humans clearly do not. Beanlands and Duinker (1983), amidst many sensible suggestions on other topics, may have therefore, erected a conceptual framework for EEM that may indirectly render impossible any meaningful prediction of impacts.

Furthermore, both the type of holistic environmental experiments proposed by Beanlands and Duinker (1983) and the experimental EEM which forms a central part of those experiments can only have value if they are performed with the scientific and statistical rigour that recent commentators on EEM have demanded (Conover, 1985). Yet, if the demands of the scientific method (sensu lato) and of statistical techniques are applied rigorously, holistic ecosystem experiments will almost always be impossible. There are three interrelated reasons for this conclusion.

First, Beanlands and Duinker's (1983) demand for tests of hypothetical impact predictions implies the use of the Scientific Method sensu stricto. As outlined in Section 2.4.2, this may not be appropriate to environmental field research. In the presence of uncertainty, research should be carried out distinct from monitoring by the biometric method outlined in Section 5.2. When studying perturbed ecosystems, these null hypotheses should always take the form of "there will be no effect", which makes them inappropriate as predictions for EIA.

Second, it is also difficult for any field experiment to yield valid causal inferences (Sections 5.3 and 5.4). When studying the major perturbations of the environment resulting from developments, random allocation of treatments to experimental units is often impossible. Much of the biometrical method is thus unavailable to EEM, at least in so far as an investigation of causality is concerned. Yet, no scientific method or research can have much value if it cannot elucidate the causes underlying its observations.

Third, in the absence of valid treatment replication, valid biometrical inference is all but impossible since there are no data with which to estimate the appropriate error term (Section 5.4). On this ground alone, no scientific method is appropriate to most EEM designs.

In short, holistic experiments and experimental EEM would be a very valuable supplement to environmental management, if they could be performed. They very rarely can be.⁴ Even if they could, they would only be a supplement to environmental management.

Current Canadian monitoring policy should, therefore, be changed to remove that portion of the present conceptual framework. The emphasis on scientific and statistical rigor should change at the same time. Clearly, statistical

⁴ It is possible, however, to conduct small-scale field experiments with replicated treatments to study the effects of oil on several levels of the biological hierarchy simultaneously (Lane et al., 1987b; Lane et al., 1987a; Crowell and Lane, 1988).

techniques should only be used correctly; employed in other ways, they can be misleading. Rigorous limits to their use are, however, likely to be narrower than correct ones, since a large body of statistical research has shown that these methods can yield valid results when some of their assumptions are violated. Similarly, knowledge gained through application of the scientific methods should be employed during monitoring activities. Clearly, this application should be made carefully and correctly with a full understanding of the limitations of the knowledge and the methods. This is a normal part of scientific activity, but it is very far from a rigorous use of the Popperian Scientific Method.

3.1.3 Delimitation of EEM

It is one of the truisms of EEM that no program can measure every environmental variable. Many arguments have been advanced for restricting monitoring to certain types of variables, measuring the effects of only selected anthropogenic inputs. These schemes can be as simple and elegant as the "Sediment Quality Triad" (Long and Chapman, 1985; Chapman, 1986) or as complex and comprehensive as Maritime Testing (1985) Ltd.'s (1987) suggestions. Most claim some scientific basis for their selection of variables, although in many cases, this can be questioned. LGL Ltd. et al. (1985), for instance, stated that offshore oil production can only have an important environmental effect in the near-field, and then only in the benthic system. Empirical basis for this conclusion was the Gulf of Mexico field studies reviewed in Chapter 2.

Scientific information should, of course, always be used, when available, to delimit the range of variables monitored and hence to improve the efficiency and lower the cost of the EEM program. It is clear, however, that current knowledge is insufficient for much scientific restriction of monitoring offshore; neither the effects of oil production nor which environments can be safely left unmonitored are known. It might be more appropriate to delimit the range of variables monitored by an explicitly regulated withdrawal of EEM from certain environments and types of variables.

This is, of course, approximated in practice. There has, for example, been no suggestion to date that ambient aerial concentrations of aromatic hydrocarbons should be monitored downwind of offshore oil development, nor that the effects of a gravity-based platform on surface wave patterns should be recorded. The platform must, nevertheless, have an effect in each of these environments. Cole (1983) suggested that the effects of offshore oil production should be monitored inshore, on the rational, though not traditionally scientific grounds, that that is where any effects will impinge on human affairs (McIntyre, 1984, 1986; Segar et al., 1987). Similarly, Wolfe (1987) suggested that biological variables should only be monitored in areas where there is known to be moderate chemical contamination, although it cannot be known whether or not less-than-moderate contaminant levels are still harmful. Conover (1985) proposed that EEM should not be undertaken unless: (1) an unacceptable degree of uncertainty in the expected effects exists, (2) the issue is sufficiently significant to require the expense of monitoring, and (3) the benefits of monitoring for environmental protection are expected to outweigh the costs.

When scientific knowledge does not serve to limit sufficiently the range of monitored environmental variables or anthropogenic inputs, that limitation should be openly based on policy decisions. It should not be founded on arguments that no effect is possible in certain environments. Such decisions might be made on many rational bases but the most obvious is program cost. If an environment or a variable is expensive to monitor and if there is reason to conclude that it is unlikely to be affected or that any effect is unlikely to impinge on human interests, EEM could be withdrawn from that environment. More specifically, if a particular variable cannot be monitored in such a way that a socially important change is detectable, while constrained within an acceptable program budget, then EEM should be withdrawn from that variable as an explicit budgetary decision (Gray et al., 1980). If this approach was followed, EEM programs would be reduced to acceptable sizes.

3.1.4 Definition of Causality

Causal relationships in environmental effects cannot usually be demonstrated by the conventional experimental/statistical approach (Sections 5.3 and 5.4). This is not something that can be overcome by some new model or design. It is one of the few absolute impossibilities of EEM.

Scientific conclusions of causality may, nevertheless, be possible but there can be a simpler route: define causality by arbitrary regulation. In the North Sea, the British government has provided, through legislation, for the fishing industry to obtain compensation for gear damaged or fouled by oil. The fishermen cannot, however, pursue legal actions for compensation for any claimed damage to the fish resource caused by offshore oil operations. In effect, the British government has declared that no changes in fish resource status can have been caused by offshore oil production.

This approach could be used in more subtle ways. All hydrocarbons entering the marine environment within one kilometer of a production platform could be declared, for legal and management purposes, to stem from the platform or associated activities. Changes in fish resource status in any area where the sediment hydrocarbon concentration does not exceed an average value for Atlantic Canadian offshore areas could be declared not to be caused by oil pollution. Such decisions are not compatible with a research orientation to EEM, but used wisely they could greatly simplify environmental management. Of course, they move key decisions from the scientific to the management arena but if there is no scientific basis for the decision, because of a lack of information or prohibitive cost of monitoring, such an explicit move may be appropriate. Where science can aid the decision makers, scientific advice should be sought.

3.2 Statistical Possibilities

The solutions to the quantitative problems of EEM are far from being exclusively statistical, but statistics constitute a key quantitative component of any EEM program. Improved statistical approaches can, therefore, be expected to lead to more efficient EEM. The present section is a

discussion of the major directions that statistical developments for EEM should take.

3.2.1 The Need for Inferential Statistics

Having discussed the inapplicability of experimental EEM, the fundamental objective of remaining EEM activities is to supply information on the basis of which decisions can be made. The data gathered will inevitably be variable and so the supplied information must be based largely on statistical analyses. Furthermore, if the information is to be useful to the decision makers it must be interpretable.

The solution is to base the advice on the relatively-objective, inference-testing statistics, such as ANOVA. Hurlbert (1984) rejected this solution and argued for the use of subjective interpretations of graphs and tables, such as Tukey's (1977) Exploratory Data Analysis (EDA), in preference to inferential statistics. Exploratory Data Analysis is a collection of techniques for displaying and analysing data in such a way as to uncover subtle or hidden trends, without the use of traditional inferential methods. Methods include novel descriptive approaches to graphing, charts and tables, and multivariate displays. Hurlbert (1984) concluded that some successful ecological research, such as the Hubbard Brook deforestation experiment and much of the work in the Canadian Experimental Lakes Area, had been carried out without conventional statistical inferences. Green (1984) agreed with him. In contrast Stewart-Oaten *et al.* (1986), Carney (1987), and the present authors argue that for EEM confirmatory, decision-oriented inference-testing methods are essential.

Part of this divergence of opinion stems from Stewart-Oaten *et al.*'s (1986) monitoring-oriented interpretation of Hurlbert's (1984) ecological paper (Section 5.4.3). In ecological research, the final conclusions are typically partially-subjective extrapolations from the raw quantitative results, supplemented by other ideas drawn from the literature and a measure of deductive reasoning. Furthermore, research can be valid and successful if it examines only the major effects of a treatment, so long as its aims are appropriately framed. The subjective interpretation of data can be entirely adequate for these purposes. In monitoring, however, the final conclusion is a decision to proceed with, or terminate, some activity. Erroneous decisions can cost some stakeholders many millions of dollars. Also, subtle minor effects of impacts can be socially important. EEM should be capable of detecting such changes. If it is not, at least an upper bound on the magnitude of an undetectable change must be calculable. In agreement with Stewart-Oaten *et al.* (1986), it is suggested that EDA alone is insufficient for these purposes.

EDA does, however, have an important role in the preliminary analysis of any data set (Tukey, 1977; Stewart-Oaten *et al.*, 1986). Furthermore, with the highly-variable data expected from EEM, more advanced analyses, such as ecosystem simulation models should be based upon a good understanding of the specific ecosystem. It can be useful to analyze the data subjectively before undertaking such complex modelling as an aid to understanding, and then only accepting the results if the subjective and objective approaches are mutually

supportive. Such a procedure is not, however, often justified with a simple analysis such as ANOVA.

Hurlbert's (1984) preference for the EDA approach was founded on his conclusion that valid ANOVA is almost impossible in the field of ecology. He argued, in effect, that the subjective biases of an interpreter of graphs are more transparent to users of the results than are the hidden errors of an inappropriately applied ANOVA. Stewart-Oaten *et al.* (1986) argued for the reverse position. The choice is not between EDA and inappropriately applied ANOVA, but between a valid test of inference and the termination of EEM. If no such valid test is possible, EEM is not very useful for environmental management.

Green (1979) proposed an elaboration of the EDA approach for EEM. He suggested that statistical tests are unnecessary if sufficient data are collected; any effect should be clear from inspection of means and confidence limits alone. Given a reasonable amount of data, a valid inference test can detect relatively small effects; that is, smaller than the variance of the data. Visual inspection, on the contrary, cannot be expected to demonstrate effects reliably that are small relative to the variance. Yet, no matter how many data points are collected, the underlying variance will not change. Furthermore, ANOVA can partition the observed effects between various causes. With advanced models this can be done with considerable subtlety. No subjective inspection of the data can hope to do as much.

The improvement in any statistical analysis through the collection of extra data follows a law of diminishing returns. To obtain a marked improvement by this means would, therefore, require a considerable increase in the field survey program. For offshore EEM, subjective interpretation of extra data, as suggested by Green (1979), would require a much larger monitoring budget than would be needed for analysis by an efficient inference test. For all of these reasons, increasing the sampling effort so as to reduce reliance on inferential statistics is not recommended.

In summary, we follow Stewart-Oaten *et al.* (1986) and Carney (1987) in suggesting that EEM must culminate in an inference test. This test cannot demonstrate causality (Section 5.4 and 5.5), but such a test can demonstrate that something has happened that must have had a cause. The test should be valid for the situation in which it is used.

3.2.2 General Linear Model (GLM)

Although the overall culmination of an EEM program must be in the form of a statistical hypothesis test, it is clear that no commonly available, packaged test is fully adequate. The desired analysis must, at the very least, capture the information in the spatial and temporal arrays of sampling stations. This cannot be done, for the types of arrays used offshore, by any *t*-test or factorial ANOVA. A "custom-designed" test is required.

The obvious basis for developing such a test is the General Linear Model (GLM). GLM presupposes no more than that the observations result from the (additive or linear) summation of a series of terms, one or more of which is a

random error term. ANOVA, ANCOVA and linear regression are all special cases of GLM. Carney (1987) provides a brief, clear and accessible explanation. When past EEM programs have progressed beyond t-tests and correlations, they have used adaptations of GLM. Disregarding the use of linear regression for the reasons set out in Section 5.3, the simplest GLM-derivative used in EEM has been Green's (1979) "optimal design" ANOVA. The failings of this have been described by many authors (Skalski and McKenzie, 1982; Hurlbert, 1984; Stewart-Oaten et al., 1986) and need not be repeated here.

A few more complex ANOVA designs have been used in EEM. Bernstein and Zalinski (1983) proposed:

$$Y_{ijkl} = U_{ij} + T_{ik} + V_{ijk} + e_{ijkl}$$

Where: Y_{ijkl} is the measured abundance in the l th replicate sample collected at the k th sampling time, at the j th station under the i th condition, T_{ik} is the effect of time, V_{ijk} is the interaction among times, stations and conditions, and e_{ijkl} is a random error term. An impact would be shown by a significant V_{ijk} term (Bernstein and Zalinski, 1983). This design is attractive, in that it incorporates temporal and spatial variability. It is also certainly imaginative and that is presently a necessary criterion in EEM analytical designs. The central error in Bernstein and Zalinsky's (1983) design is, however, clear. At any one time the value of condition is fixed for any one station: condition is always unimpacted at a control station whereas at an impacted station it is unimpacted before the development and impacted thereafter. The design thus completely confounds time, space and condition and cannot yield a meaningful test of their interaction. The design also makes no provision for valid replication. Bernstein and Zalinski (1983) employed what Hurlbert (1984) would term temporal pseudoreplication; Millard and Lettenmaier (1986) have noted other errors in this design. The confounding is, however, sufficient to reject Bernstein and Zalinski's (1983) ANOVA design.

Hines et al. (1987) performed two analyses, one using an ANOVA design similar to Green's (1979) whereas the other used analysis of covariance (ANCOVA). For the latter, the seasonal cycle was represented by a sine curve and transformed to linearity by an inverse trigonometric function ($\sin^{-1}$ or $\cos^{-1}$). This was then used as the covariate to adjust for seasonality in the data. The other terms in the ANCOVA were conventional ones. A sine curve is clearly a crude model for the seasonal cycle in most data sets. Moreover, the primary problems with Green's (1979) ANOVA design have nothing to do with seasonality, so Hines et al.'s (1987) ANCOVA is still insufficient. Nevertheless, the idea of compensating for some types of continuous variation by including them in the analysis as covariates has merit. The covariates must bear a linear relation to the measured variable, but this can often be ensured by transformation. This includes not just the conventional logarithmic and power transformations, but also the trigonometric ones used by Hines et al. (1987). any of the multivariate ordination methods (see Digby and Kempton, 1987, or other standard texts) or even an empirical, study-specific technique.

Mathur et al. (1980) used a quite different GLM-derivative model in an EEM study, one based on multiple linear regression. As published, their model is

felt to be defective mathematically, but the essence of their approach may be useful. They performed a complex multiple linear regression, itself a form of GLM, structured so as to predict pre-impact abundances at the impacted stations from various data, including pre-impact abundances at a control station. They then added additional terms to their model to convert it into ANCOVA and, in essence, tested for deviations between the measured post-impact abundances and equivalent predictions based, in part, on post-impact measurements at the control station. Mathur et al. (1980) were able to predict their pre-impact abundances with high confidence (R^2 between 0.88 and 0.92) and if the factors driving the regression relationships can be assumed to be stable through time, the approach promises a much more representative control than any station separated in time or space from the impacted stations could be.

The remaining GLM-derivative approach that has seen much use in EEM is the Control-Treatment Pairing (CTP) ANOVA pseudodesign discussed in Section 5.4.3 (Eberhardt, 1976; Thomas et al., 1981; Skalski and McKenzie 1982; Millard et al., 1983; Palmer and Mackenzie, 1985; Millard and Lettenmaier, 1986; Stewart-Oaten et al., 1986). In this family of designs, measured values at control stations are subtracted from those at impacted stations, and the ANOVA is performed on these differences. If this subtraction is performed on logarithm-transformed data, as is usually done, it is equivalent to calculating the ratio of the variable as measured at the two stations. These CTP designs have great merit, though as discussed in Section 5.4.3, their statistical validity is still not proven. They may never be shown to be generally valid, in which case, measurements of temporal correlations in the data will always have to precede a CTP ANOVA.

Mathur et al. (1980) have criticized the CTP approach on the grounds that there is no unique way to define the inter-site differences. Other authors have pointed out that aberrant data from a single control station affect the analyses of every impacted station, with rather unpredictable effects on the resulting probabilities and significance. A simple CTP ANOVA design, such as Stewart-Oaten et al.'s (1986; Figure 5.4), is less than adequate for offshore EEM. Nevertheless, just as covariate terms may have value in a GLM-derivative model for EEM, so also may CTP terms.

An alternative to the CTP approach has been proposed but not yet used (J. A. Carter, pers. comm.). In this, the differences are calculated not between stations at the same time, as in CTP, but between impact states (pre- and post-development) at the same station. CTP removes spatial correlations by taking differences but it must rely on temporal repetition for its (pseudo) replicates, and hence is vulnerable to temporal correlations in the data. The alternate pseudodesign removes temporal correlations through its subtraction step but, in the absence of valid treatment replication, is vulnerable to spatial correlations in its error term. The choice between these two approaches may, therefore, depend on which of spatial and temporal correlation is largest in a particular data set. The alternate pseudodesign does at least have the major advantage that there are usually many stations in an EEM array but only two impact states: pre- and post-development. Mathur et al.'s (1980) objection of the non-uniqueness of CTP pairs is thus overcome.

In all of these discussions, it has been assumed that the model will be designed in some manner to test the difference between an impacted area and a control which, if not pristine, is at least unaffected by the monitored production platform. The Central Gulf Platform Study introduced an alternative idea in which the different effects of two levels of anthropogenic stress would be contrasted. In effect, they recognized that a literally unaffected control station cannot be found within the vicinity of a production platform.

Their "two-dose" model avoids one major problem facing the analysis of offshore EEM data. It cannot, of course, do so without some costs to the inferences that can be drawn from the analysis. For example, it is impossible to infer the presence, absence or degree of change from background conditions with a "two-dose" analysis. It can, therefore, only be used in EEM if the monitoring objectives avoid reference to that background. Useful inferences may also require that the ratio of the "dose" levels is known. If the effect is known to be caused by hydrocarbons and the loading at the lower-dose station is known to be one-tenth that at the high-dose one, a useful conclusion could be drawn from any detected difference in effect levels, at least if linearity of response can be assumed. If, however, the proximal cause of the effect is unknown if the lower dose is a large portion of the higher one, or if the dose-response relationship is markedly curvilinear, a two-dose model will yield results of little relevance. Thus, the approach can have great value but is not universally applicable.

Clearly, none of these GLM-based models is adequate for offshore EEM as they are presently formulated. There is a need for statistical development to derive an appropriate model. There are, however, strong indications that a model could be developed, perhaps incorporating (transformed) distance from the production platform as one covariate, various ecological predictor variables as others, and some pair-differences as data.

No such model, however, can suggest causality in the absence of valid treatment replication (Section 5.4; Stewart-Oaten et al., 1986). The other problems of an absence of valid replication also remain, particularly the influence of correlated errors on probability estimates and the lack of a valid divisor in F ratios (Section 5.4). These problems may, however, be surmountable by appropriate designs. Millard et al. (1985), for instance, claim that temporal correlations are insufficient to invalidate their CTP design, at least in some data sets.

Finally, Green (1979, 1984) has suggested that multivariate ANOVA (MANOVA) could be used so that a single analysis could incorporate all of the monitored variables at once. There are several problems with this suggestion. Not all of the variables will require the same station array, nor the same analytical model. If ten variables are monitored and nine show no environmental effect, it is still necessary that a real effect in the tenth be detected. MANOVA, however, uses data from all ten to determine the aggregate probability of an effect. A moderately-significant result for one variable can, therefore, be concealed by the lack of an effect in the other variables. In general, for EEM, a series of univariate ANOVA's is preferable to MANOVA.

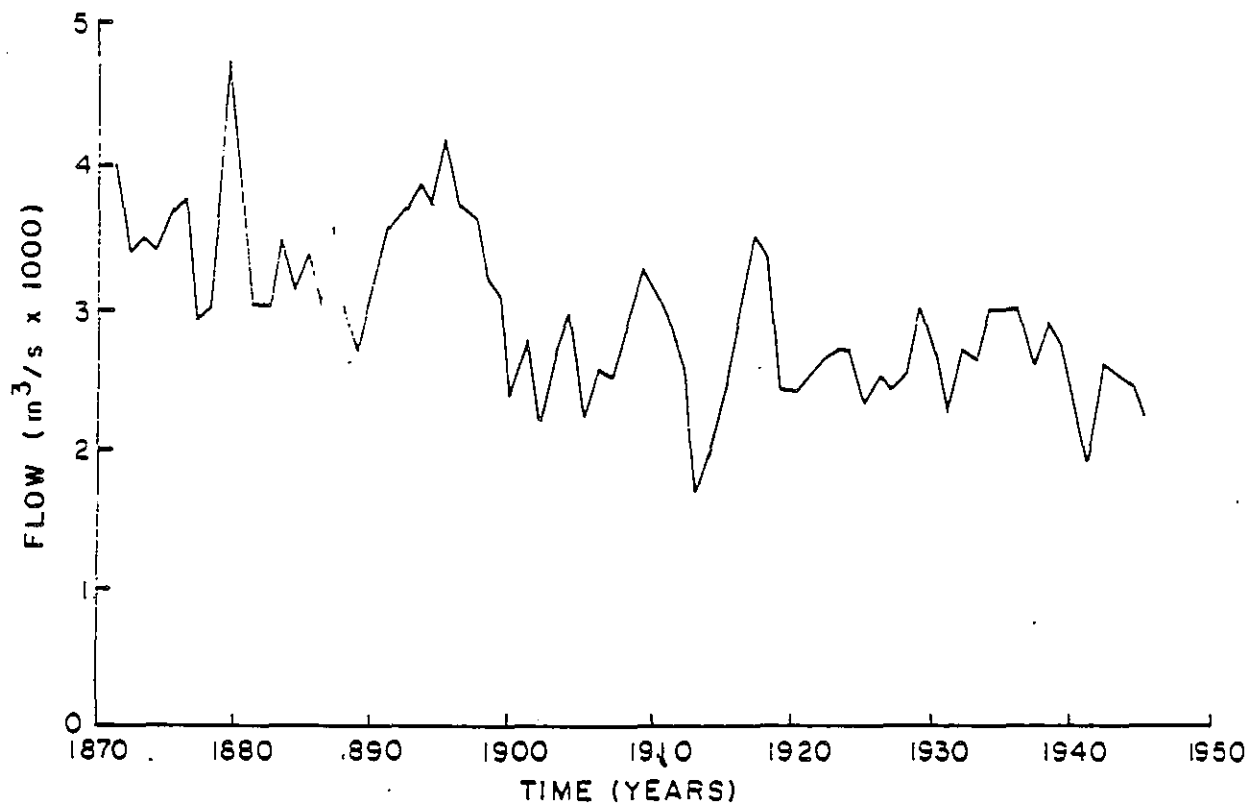
3.2.3 Box-Jenkins Models and Intervention Analysis

Box-Jenkins Models are specifically intended for the analysis of time-series data. They differ from GLM in that some of the terms involve internal multiplicative relationships, although the various terms are still additively related to each other. Hipel et al. (1978) proposed that EEM (their "assessment of environmental impacts") should be based around Intervention Analysis, a derivative of Box-Jenkins modelling in which extra terms or "intervention components", are used to describe the effects of the intervention of external factors on the time series. For EEM, of course, the aim is to see whether there is a significant intervention component corresponding in time to the commencement of the monitored development. This approach has occasionally been applied to monitoring in practice (Van Latesteijn and Lambeck, 1986).

Intervention Analysis is not, however, very powerful. Lettenmaier et al. (1978) have made a detailed power study of the technique. From their results, it appears that detection of a change equal to 25% of the standard deviation (itself often equal to the mean with marine data), with $\alpha = 0.05$ and $\beta = 0.30$, requires 10,000 data points over five years. The chance of not detecting such a change, β , is still 30%. Furthermore, it is important for all Box-Jenkins Models that the data points are equally spaced in time. For an offshore oil development such as Hibernia, this means that one measurement must be made every four hours at every station for five years. Clearly, this is impractical when sampling from a surface vessel. It might, however, be practical if the particular variable could be monitored by an automatic, moored probe, with data telemetry to the production platform; this level of sophistication is possible for many chemical and physical variables.

There are, however, still two problems with any time-series analysis approach for EEM. First, the only indication of causality that it can provide is the coincidence in time between the development and a change in the data series. Indeed, the ability of Intervention Analysis to distinguish small, but crucial, differences in time between these events is questionable.

Second, Box-Jenkins Models can only detect cyclic phenomena in the time series. If that series is relatively short, as in the 20 years of data from offshore oil development when compared to the 40-year Russell Cycle (Section 5.5.2), long-term cycles will appear as monotonic trends in the data. Intervention Analysis will interpret such a trend as a change in the mean conditions between pre- and post-development periods, which is indistinguishable from an effect of the development. Hipel et al.'s (1978) primary illustrative example, the time series of Nile River flow measured at Aswan between 1870 and 1945, is a good example of this (Figure 3.1). Their Intervention Analysis showed a 20% decrease in mean flow coincident with completion of the Aswan Dam in 1902. The data are also consistent with a long-term decline in flow beginning in or before 1870, and perhaps slowing after 1910 or 1920. Such trends could clearly be incorporated into the model by adding a linear term. To avoid confounding the trend with any environmental impact effect, this would require a design with control stations, which goes beyond the realm of Box-Jenkins Modelling.



AVERAGE ANNUAL FLOWS OF THE NILE RIVER
AT ASWAN

Furthermore, the relatively short temporal span of an offshore EEM program means that any Intervention Analysis would have access to data from, at most, 20 annual cycles and often only five or 10. This is insufficient for the pseudo-random inter-annual variability (Section 5.5.2) to be interpretable as multi-year temporal cycles. Confounding of inter-annual differences with pre- and post-development period differences is inevitable.

Intervention Analysis per se is, therefore, of limited use for offshore EEM, being applicable at most to physical and chemical parameters close to the platform. When developing the GLM-derived models suggested above (Stokoe et al., 1989), however, consideration should be given to incorporating some Box-Jenkins terms. Cyclic temporal variation may be better modelled in this way than by inverse trigonometric transformation into a linear covariate.

3.2.4 Non-Parametric Statistics

Thomas et al. (1978) suggested using non-parametric methods for EEM analysis, and more recently it has been suggested that such methods could overcome the major statistical problems facing EEM. This is questionable. Non-parametric methods differ from parametric ones in only one way: their results are not affected by the statistical distribution of the data. In all other respects, the non-parametric techniques are no more than analogs of parametric methods. Spearman's ρ , for example, is a distribution-independent correlation coefficient that hardly differs from Pearson's r . It is calculated in exactly the same way, except that it requires rank-transformed data. The Kolmogorov-Smirnov, Mann-Whitney and Kruskal-Wallis tests are all rather crude approximations to a distribution-free, one-way ANOVA. Thus, the choice between non-parametric and parametric techniques has little relevance to the major problems facing EEM, such as the lack of valid replication.

If EEM produces data with a distribution that does not, and cannot be made to, meet the requirements of the preferred parametric methods, non-parametric ones should be used. There are two disadvantages to being forced into this position however: non-parametric methods are almost invariably much less powerful than their parametric analogs, and no non-parametric technique can approach the subtlety possible with a parametric GLM approach. The lack of power results in an inflated β , with serious consequences for EEM (Section 5.2.4). The lack of subtlety prevents the use of the "customized" models advocated in Section 3.4.2 and reduces EEM analysis to the non-parametric equivalent of few t -tests and correlations.

It is, therefore, highly preferable to use parametric statistics whenever possible, transforming the data as necessary to permit this. Clearly, the chosen or purpose-designed statistical model must be carefully explored, so that both its robustness to poorly distributed data and its responses to data to which it is not robust are known. Simple ANOVA can be very robust to non-normality (Glass et al., 1972), and if the EEM models prove to be the same, the non-parametric approach may be entirely unnecessary (Green, 1979).

If the ANOVA approach proves untenable, there is still another alternative approach (Green, 1979). It is possible to generate synthetic data with the statistical distribution expected for the EEM data and containing known

"environmental effects". Such data can be analyzed by the chosen technique, in a Monte Carlo experiment, and the results used to prepare a probability table which will differ from the true percentiles of the F distribution. When the model is applied to real data, the resulting F ratio can be compared to the synthetic probability table, rather than to the F distribution, in order to determine its significance. This is a somewhat crude but effective method that allows the application of complex GLM designs to unevenly distributed data (Green, 1979).

3.2.5 Type II Errors in Monitoring

Throughout this report, the difference between Type I and Type II errors, the false rejection and false acceptance of null hypotheses, and their respective probabilities, α and β , has been stressed (Sections 2.3.3 and 5.2.4). The emphasis of research on α and Type I errors has been contrasted with the proper concern of monitoring with β and Type II errors. It is only necessary here to stress, once again, that the acceptance of a null hypothesis in EEM, in the absence of evidence that β is suitably small, only demonstrates the inadequacy of the monitoring program.

The dichotomy between the statistical objectives of research and monitoring was recognized in the early EEM literature (Eberhardt, 1976, 1978; Gray et al., 1980) but has been overlooked in practical applications. In consequence, there has been considerable confusion between the proper conclusion of many studies, that "no evidence of an adverse environmental effect was found", and the more readily understood, more comfortable, but erroneous: "evidence was found that there was no adverse environmental effect". Carney (1987) and Millard (1987a,b) have recently returned to this theme. The former gave a particularly clear exposition of the issue whereas the latter emphasized the seriousness of past failures to recognize the importance of β in EEM.

Since research properly emphasizes Type I errors, considerable attention has been given to appropriate critical values of α (often 0.05, but see Section 5.2.3). β has, in comparison, been ignored. For EEM, Eberhardt (1978) pointed out that developers will usually desire α to be very low so as to minimize the chance of being falsely blamed for degrading the environment. The regulators, on the other hand, will often wish β to be very low, so that real damage will not pass undetected. Setting $\alpha = \beta = 0.01$ would, however, require impractical sampling frequencies offshore; where both the sample variance and the costs of sampling are high. There have been suggestions that appropriate values of α and β could be set by Decision Theory (Millard, 1987b).

It might be desirable for EEM to be founded on a statistical theory which placed more emphasis on Type II errors, in a manner analogous to the emphasis on Type I errors seen in conventional research-oriented, inference-testing procedures.

3.2.6 Null Hypotheses in Monitoring

When inference-testing statistics are used in research, it is necessary to apply Occam's Razor when formulating null hypotheses (Section 5.4.2). Were

experimental EEM possible, the same approach would be appropriate. For verification EEM, however, it is not. The research approach must always lead to a null hypothesis of the general form:

H_0 : There is no effect.

Meanwhile, the procedures of environmental impact assessment will be used to formulate a prediction like:

There will be effect x , of magnitude y , at location z .

To be useful, verification EEM must test this prediction, not the research-oriented null hypothesis. It should usually perform a one-tailed test and may need to test whether the magnitude of the effect exceeds y by an unacceptable amount. Thus, the correct null hypothesis might be of the form:

H_0 : At location z , effect x has a magnitude no greater than $(1 + \delta) y$, where δ is a safety factor set by the same management procedures that are used to determine that magnitude y is itself acceptable.

Besides being the appropriate structure for a verification EEM null hypothesis, this has one major advantage. Since β is high in most EEM programs, null hypotheses will often be accepted. If those hypotheses have been correctly formed, as above, this false acceptance leads to a conclusion that "no evidence was found that environmental effects are no worse than expected" which is at least less misleading than "there are no environmental effects".

If this approach appears to be too great a perturbation of statistical methods, it may be noted that it is merely a crude adaptation of Bayes's Theorem:

Posterior Probability = Prior Probability x Likelihood

In EEM, assuming that an Environmental Impact Statement has been appropriately prepared, the effect hypothesis with the highest prior probability must be the impact prediction in that statement. If the testing EEM program is too weak to distinguish among various hypotheses, then:

Posterior Probability = Prior Probability

and the most probable hypothesis after EEM remains the impact prediction.

3.2.7 Spatial Smoothing, Geostatistics and Kriging

In the presentation and display of spatial data a good deal of interpolation and smoothing is invited by the relative paucity of sampling points in spatial domains of interest. In the past two decades increasing use has been made of automatic interpolating methods (Davis, 1987), such as spline techniques (Eubank, 1987), linear and polynomial techniques and the like. Since the early 1960's a body of spatial interpolation techniques referred to as geostatistics (Journel and Huijbregts, 1978; Henley, 1981; Ripley, 1981) has

gained wide acceptance. Beginning with problems of estimating the extent and quality of ore reserves, South African mining engineers and French mathematicians developed techniques collectively called "kriging" (after D.G. Krige, one of the pioneers of the techniques).

Kriging is based on theories of regionalized variables that take advantage of the correlation structure of spatially-distributed random variables for interpolating and areal estimation of such variables. Depending on some fairly restrictive assumptions, not only point estimates but error intervals about them have been calculated. Such methods are finding rapidly increasing application in several of the physical sciences: hydrology, groundwater, air pollution, among others (Delhomme, 1979; Cooper and Istok, 1988a,b; Lefohn et al., 1987; Wolka and Austin, 1988; Zirschky and Harris, 1986; Dingman et al., 1988; Gilbert and Simpson, 1985).

There have been claims about the "newness" and merits of these techniques, and there has recently been a considerable amount of debate -- some of it rather subjectively based (Philip and Watson, 1986; and respondents: Journel, 1986a; Krige, 1986; Shrivastava, 1987; Matheron, 1986; Shrivastava, 1986). The debate, however, has served to sharpen the focus on the consequences of the invoked assumptions and the extrapolation of claims of relevance.

There has been attention paid to robust estimation in kriging (Bardossy, 1988) and the use of qualitative, judgmental, or "soft" information in applying it (Journel, 1986b, 1988). Problems associated with transformations to the normal distribution, in order to satisfy underlying assumptions, have led to subsets of kriging denoted by indicator variables and disjunctive framework. Comparisons of alternative methods have been studied in specific instances (Pueente and Bras, 1986).

Ripley (1981) has also presented kriging in the context of time-series analysis. We suggest that such approaches, combining spatial and temporal frameworks, would facilitate smoothing, interpolating, and interpreting environmental monitoring data. No absolute position should be taken regarding their philosophical correctness. Rather the arguments should be noted and tested numerically, using Monte Carlo or other error analysis techniques. Extensions of kriging applicable to environmental management decisions that explicitly take into account power considerations have been suggested by Journel (1984):

"Assessment of spatial distributions of pollutant concentrations is a very specific problem that requires more than blind mapping of these concentrations. Not only must the criterion of estimation be chosen carefully, but also the valuation of the potential error of estimation calls for a much more meaningful characteristic than the traditional [kriging] estimation variance. Finally, the risks, of making wrong decisions must be assessed.

3.2.8 Sampling and Station Arrays

The various disadvantages of the commonly-used radial transect station arrays have been discussed (Section 2.3.1), and we have already recommended giving more attention to Hartley's (1982) suggestion of using grid arrays. The statistical implications of the grid approach are unclear, since it has so rarely been used, and they must be explored before the approach is applied. Grids would provide suitable data for kriging smoothing (Section 3.2.7). The various optional features of grid designs must also be investigated, including: whether the grids should have rectangular, circular or elliptical boundaries; whether the stations should be regularly or randomly placed and whether station density should be constant or should increase toward or away from the platform. It is suggested that the implication of these alternatives be examined before the station array for offshore EEM is chosen.

Gray (1980) and Gray et al. (1980) have suggested that EEM surveys should be spatially stratified. This idea should be explored in conjunction with the study on grid arrays. Effective stratification will certainly reduce the sample numbers required and, hence, the program cost. It is much less certain whether effective stratification is possible in the offshore environment, given the present lack of knowledge of variation patterns and the need for EEM to simultaneously survey a suite of variables. More theoretical and field research is clearly needed before full advantage can be taken of the economies possible through this approach.

The problems of sampling, given the high degree of intra-annual temporal variation, have been outlined in Section 5.5.3. Some variables may be effectively monitored at a single date each year. For most others, limiting EEM to annual or quarterly surveys would prevent the program from reducing β to acceptable levels for any but gross environmental changes. Monthly sampling, or better still lunar monthly (13 times per year), would often be sufficient. Harrington (1977) discussed the relationship between sampling frequency and averaging time.

Frequent survey repetition is not suggested. Instead, the desirable variables to be monitored should be selected through an integration of policy concerns, ecological modelling, ecotoxicological considerations and the like. The spatial and temporal variation patterns of the chosen variables should then be documented by pilot studies. Subsequently, appropriate temporal sampling frequencies can be determined for each variable. If the required frequency for a particular variable is such that monitoring cannot reduce β to a useful level within the constraint of a realistic budget, then that variable must be deleted from the monitoring program. This aspect of the design process should be pursued iteratively, with the ultimate aim of obtaining a minimum-cost but still effective EEM program.

These steps may show that the only EEM possible within an acceptable budget comprises an annual survey confined to those variables with minimal intra-annual temporal variation. They may also show that the minimum effective EEM program requires weekly surveys. There is, however, a reasonable probability that some intermediate program will have both an acceptable cost and adequate effectiveness.

3.2.9 Statistical Development

The failings of present statistical methods for EEM have been discussed at length in Chapters 2 and 5. There are not as yet any fully developed methods appropriate to offshore EEM. Some statistical model development is, therefore, needed to support EEM. Furthermore, since field data must be collected appropriately for particular statistical methods (Section 5.1), that statistical development should proceed before pre-development field surveys can be undertaken.

Two areas for development are particularly noteworthy. First, improved ANOVA designs, or rather General Linear Model derivatives, are required. Ideally, these would allow a test of pre- and post-development differences, while incorporating the spatial information in the station array and controlling for spatial patchiness and temporal cycles. Developing such models will require considerable insight. Second, these models or whatever other inference tests are to be used must be carefully examined. Their power must be known, so that optimal sample numbers can be calculated. Their robustness or susceptibility to a range of data problems must be explored, so that they can be validly applied without excessive sampling. This statistical development is not trivial. Indeed, it may prove to require a substantial effort.

3.3 Conceptual Framework and EEM Design Possibilities

The purpose of this report has been to summarize the quantitative aspects of the conceptual framework for the EEM design and its implementation for monitoring oil production activities at offshore sites. Although it is outside the scope of this report to provide that conceptual framework, such a framework is needed. The conceptual framework must be made explicit at the beginning of the design phase. Many of the areas discussed in the report relate to the eight aspects of this conceptual framework. These aspects were listed in Chapter 1. The following discussion is based on that list, but it is not meant to be a comprehensive treatment of each aspect since the discussion is limited to mainly quantitative considerations.

3.3.1 Scoping

Much on this aspect has already been discussed in Section 3.1. In the scoping for offshore EEM, the objectives must be explicitly stated and there must be a clear distinction between the roles of science and policy. Much of this report should serve as a guide for framing objectives. At a minimum, the EEM program should be designed to detect overall environmental deterioration. It should also be designed to monitor key variables or ECARs (Ecosystem Components at Risk) as have been defined in the Generic IEE for the Northumberland Strait Crossing Fixed Link for Public Works Canada (P. Lane and Associates/Washburn, & Gillis Associates, 1988). A broader definition is used here than that given by Beanlands and Duinker (1983) for Valued Ecosystem Components (VECs).

In the preliminary scoping and also in the selection of variables, the notion of relevancy should be given serious and continual attention. Relevancy must

be defined in both a regulatory and public context. One must continually question: If there is a change in variable x of y amount, how much will it matter? To whom? How will this affect management decisions and procedures, and what are the potential trade-offs of environmental risk, benefit and cost of measuring variable x? As suggested in Section 3.1, it may be desirable and perhaps necessary, to make some decisions by regulatory fiat, and thus, restrict the monitoring of some variables. An environmental risks cost-benefit analysis should be attempted to distinguish between those variables that should be monitored and those that are best "managed" by fiat. This analysis need not be costly or time-consuming, even a rough approximation of "worst-case scenarios" could greatly facilitate identification of those variables which could provide the maximum of achievement of EEM objectives with minimum cost.

The deletion of experimental EEM from Canadian policy would remove much of the current conceptual foundation for monitoring programs. A replacement framework is, therefore, needed. The rejection of experiment EEM leads to the rejection of Beanlands and Duinker's (1983) suggestion of the intimate linkages among research, environmental impact assessment, and EEM. The present authors disagree with Rosenberg *et al.*'s (1981) less strict description of this linkage and instead suggest that Larkin's (1984) approach be followed. In this, scientific research and environmental management, including both impact assessment and EEM, are seen as separate, but parallel, activities (Section 1.3). EEM should be a tool of environmental management and nothing more. Munro *et al.* (1986) have recently outlined a role for EEM within such management, as part of a more extensive assessment audit process. Though they provide few detailed suggestions, this role is clearly that which is termed verification EEM in this report.

Within such broad terms, EEM must have clear objectives. These have been discussed by Rosenberg *et al.* (1981) and Conover (1985), both of whom recommended experimental EEM, but in other respects made valuable suggestions. From their work, the testing of impact predictions or verification EEM as the prime focus for EEM (Hecky *et al.*, 1984) is recommended. This should provide some estimate of the reliability of a prediction and, hence, of the environmental management decisions built upon it. By testing both the prediction and the decisions, it would provide the feedback process demanded by Rosenberg *et al.* (1981) and Beanlands and Duinker (1983). These tests, supplemented by attempts to predict environmental damage before it occurs via early warning EEM, will also detect unpredicted environmental effects and so aid ongoing management decisions. For this latter objective, less emphasis should be given to measuring environmental damage and more to detecting it quickly and stopping whatever activity is causing it.

It is clear, however, that every human action has some effect on the environment. It is, therefore, necessary to determine how severe an effect must be before it is considered to be of interest. For planning of EEM sampling design, this determination must be made a priori and presumably on an ecological or, for impacts on human concerns, a socio-economic basis (Dicks, 1982; Segar and Stamman, 1986; Wolfe, 1987). Once the magnitude of degradation that is important has been determined, it is essential that the EEM program has sufficient resolution to detect such changes. If this cannot

be achieved within a practical budget, EEM should be withdrawn as a management tool (Phillips and Segar, 1986).

3.3.2 Selection of Variables to Monitor

The greater part of past research effort in offshore EEM methodology has concentrated on what (not where, when or how often) to monitor. This is essentially a non-quantitative question and, hence, outside the scope of the present report. The variables, however, should be selected with an awareness of the food web and biological hierarchy of which they are a part. The FEARC panel (Paradine, 1985) suggested birds, fish, and sessile organisms be monitored at the Hibernia oil development project.

Much of the past deliberation has been directed towards choosing variables that should be easy to monitor quantitatively. McIntyre et al. (1978) and Gray et al. (1980) made early progress in this area. The Beaufort Sea Monitoring Program has been, in part, an exercise with something of the same aim. Clearly, "pathological" (Carney, 1987) responses are easier to monitor than "discordant" ones but, with the possible exception of Mixed Function Oxidase induction (Payne, 1976, 1985), the only "pathological" responses to offshore oil production yet known are simple physico-chemical measures of stress loadings.

Any selection of monitored variables based on quantitative convenience must inevitably constrain the range of environmental effects that EEM can detect. Such constraints are, however, inevitable in any monitoring program. They are not a scientific problem, provided that the lack of monitoring is not confused with a lack of effect. Every opportunity should be taken to circumvent the quantitative problems facing EEM by careful selection of the variables to be monitored, provided that the resulting monitoring program is still capable of fulfilling its objectives.

In Section 3.1, it was suggested that present scientific knowledge is generally insufficient to state which ecosystem components will not be affected by a particular development. Since complete monitoring of everything is impractical, non-scientific decisions must be taken to determine which variables will and will not be monitored. There is, of course, some extant scientific knowledge that could guide those decisions. For EEM of offshore oil production, however, Carney's (1987) recent review and this report have questioned the validity of much of that knowledge. Thus, there is an urgent need for a re-evaluation of the knowledge base given these two reviews. Such a re-evaluation is not research, but it must be a properly skeptical, scientific process.

The result of this re-examination need not be a conclusion that, for example, water-based drill mud discharges do not affect the water column or that migratory fish are unaffected and so need not be monitored. Instead, it should generate estimated probabilities of various effects occurring, the socio-economic costs if they do occur, the costs of the monitoring and the values of any actions that might be taken in response to EEM data, that is, environmental risks cost-benefit analysis. Only with such detailed scientific and technical advice can decision makers be expected to act.

3.3.3 Bounding

This is the delineation of the space and time dimensions of both the development and the environment that will be considered by the EEM program. This again is a decision formed at the intersection of science, management and logistics. Figure 1.2 illustrated four possible areas for EEM that would encompass offshore oil development activities. Bounding is intimately tied to the operable definitions of environmental change and consequently "effect" for the selected variables that will be used for EEM. For example, if the spatial bounds are so large, such as the total world ocean for a variable of interest (cod population), a change in cod at the offshore platform would not be very important for the world market but there could be an observable decline locally. Likewise, if the bounding is too narrowly specified such as an area less than the size of the base of the gravity-based platform, the "effect" would be total annihilation to the benthic community under the platform. Thus, the definition of "significant" environmental change is inherent to the concept of bounding. This is discussed in more detail in P. Lane and Associates/Washburn & Gillis Associates (1988). How bounding is done will also determine the later feasibility of application of the concepts of Cumulative Effects Assessment and Management (CEAM) (Lane et al., 1988a).

In addition to these general comments on the relationship between environmental change and bounding, there are also specific considerations concerning the natural distribution and abundance (or concentration for abiotic variables) of the variables of interest. Once the preliminary selection of variables is made for offshore EEM, it will be imperative that baseline field studies are conducted prior to oil development activities. These are discussed under point 6.

In terms of temporal bounding, EEM should be able to test the predicted effects of the termination of a project as much as those of its commencement. The last of these may not require EEM to continue unmodified for 20 years, but some variables may need to be monitored for the whole of that time period. As P. Lane and Associates Limited (1985) demonstrated, there may be some environmental effects of projects such as Hibernia that are subsequent to abandonment of the platform.

3.3.4 Adaptive Approach

The concept of the adaptive approach encompasses several subcomponents, but its basic tenet is that EEM should be designed to accommodate a wide variety of human activities, both routine and accidental that may be associated with the development, and likewise a wide variety of natural events, and that EEM should be designed from its inception to be flexible. P. Lane and Associates Limited (1988a) have provided rationale for adaptive monitoring design for hydrocarbons in marine environments. An adaptive EEM design is defined to satisfy the following four criteria:

- i) both routine and accidental events are planned for;

- ii) the monitoring plan must be robust for a wide range of events, and human activities;
- iii) the monitoring plan must be responsive and flexible but not impromptu or inconsistent in regard to methodologies and results; and
- iv) it is usually necessary to integrate data collection with modelling to achieve an optimal level of cost-effectiveness.

The Hibernia Environmental Assessment Panel called for inclusion of both episodic events and chronic effects in EEM (Paradine et al., 1985) whereas LGL Ltd. et al. (1985) specifically restricted their EEM design to the monitoring of chronic effects. Major accidents do occur at oil production platforms, as the Mexican Ixtoc I blowout, the well control problem at Venture, the Ekofisk blowout and the recent Piper Alpha fire (both the latter in the North Sea) clearly show. It would be a wasted opportunity if, in the event of such an accident, the EEM program was unable to provide a description of pre-event conditions.

Effective monitoring of oil blowouts and spills should be carried out within a few days of their occurrence. There are often severe logistical limitations in doing this. Efficient EEM cannot be designed and organized that quickly, and the monitoring of such events has to be planned in advance. Thus, any production platform's EEM program design should include rapidly-implementable plans for monitoring episodic events. The mechanisms for implementing these plans might usefully be linked to the arrangements for oil spill containment and clean-up operations.

The initial monitoring should be done so that if there were a major oil spill, all of the needed baseline data would have been collected and the procedures for measuring change after the spill would be in place and usable immediately. For such a spill, the monitoring might be much more detailed and comprehensive than the usual monitoring for routine, chronic discharges; however, the more detailed monitoring plan for major events should not only be in place prior to the spill but it should be designed so that data analysis will be consistent with the continuing routine monitoring data set. In addition, the monitoring plans should be robust to the unexpected. Over the projected 20-year development period for projects such as Hibernia, certainly there will be unexpected events that will affect the EEM program. Insofar as possible, the EEM program should not be invalidated by these types of events.

Flexibility should also be achieved by combining a "cluster of interlocking models" approach with field data collection. It is possible to develop enough understanding about a particular field situation that it is possible to substitute modelling results (for example, from a computer sensitivity analysis), for field data collection. This can be planned for at the beginning of the monitoring program and as data are collected they can contribute to the verification and calibration of models that can then be used to replace field data collection. For some variables this can be illustrated as a three dimensional response surface. This is especially important for the offshore EEM because of the cost of ship time and the extended period of

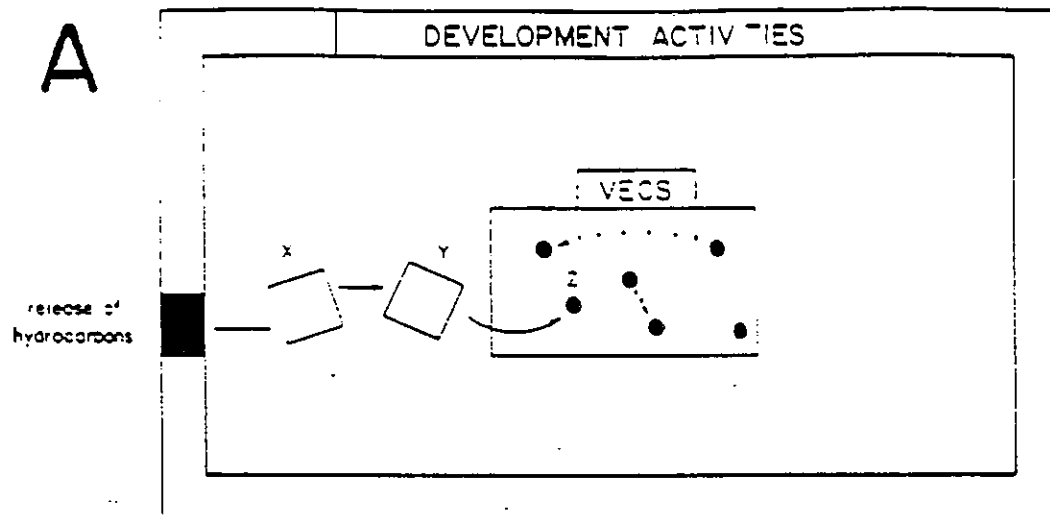
development (20 years). If "adaptability and flexibility" are not considered important to the conceptual framework at the beginning, there could be costly consequences later in the EEM program.

3.3.5 Methodology to Understand Cause and Effect

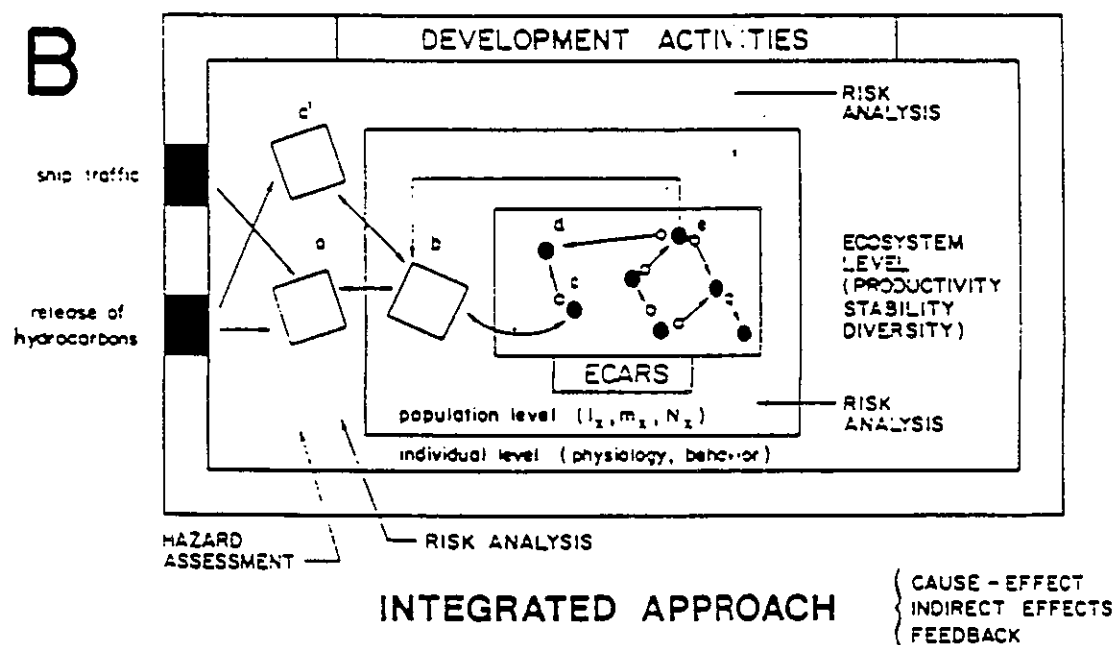
The recent volume on George's Bank (Backus, 1987) gives some indication of the scope and scale of knowledge about a marine area available to environmental managers. The designers of efficient EEM need this type of information. For example, we are still very far from having a thorough understanding of the Grand Banks ecosystem: critical parts of this ecological knowledge must eventually be expressed in the form of models. At the very least, an improved version of simple verbal/conceptual models used to support hypothesis linkages in BEMP are essential. A variety of higher level models will, in practice, be necessary for prediction, linkage description, variable selection and understanding feedback and causality. Although there is a wide range of environmental models, they will have to be adapted and/or custom-designed to provide the specific support required for EEM. Stokoe et al. (1989) summarized the state of ecosystem modelling for the marine ecosystem. Approximately 20 models are compared and grouped into four general categories. The BIO-DFO carbon model for the Grand Banks and the P. Lane and Associates Limited (1985, 1988a) network models are discussed. This summary represents the progress to date on Grand Banks ecosystem modelling that is available. Clearly more work is needed for the EEM conceptual framework.

Figure 3.2 is a comparison of the BEMP conceptualization of cause and effect (A) with an integrated approach (B). For both A and B, development activities are shown in the border around the Beaufort Sea System. For simplicity, only two development activities, ship traffic and the release of hydrocarbons shown as black squares, are illustrated. In the present BEMP conceptualization (Figure 3.2A), the development activity (or action) is causally linked through various components (x,y) to the Valued Ecosystem Components (VEC's) which are the darkened circles in the central rectangle. There has been no attempt to standardize the x's and y's in the region outside the VEC rectangle. They include various types of components, and often individual level (physiology, behaviour) and population level (mortality, density) components are mixed with energetic considerations. There is careful delineation of the development of causality from the development action through the three main ecological levels. Causality among VEC's themselves at the lower part of the food web is superficially represented; this is shown with the use of dotted lines. All causal flows are shown as one-way even though the two-way flows often predominate between pairs of components. There is no ecosystem level organization. Likewise development-induced mortality and harvest-induced mortality are not diagrammed in the appropriate causal sequence at the population level. There is no allowance for understanding the causal relationships inherent in cumulative impacts.

In an integrated conceptual plan as illustrated in Figure 3.2B, development actions first affect the individual level and potentially create individual level risk for the ecosystem components (a and a'). The ultimate risk for an individual is death, however, lower life expectancy or reproductive output can also influence the population of which it is a member. Thus, one must first



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demonstrate causal relationships with the development action on the individual level. Changes at this level whether behavioural, physiological or biochemical, etc., must be translatable into changes in survivorship (1_x), fecundity (M_x), age structure, and or abundance (N_x) at the population level (b) if they are to be observable as population effects. In the central rectangle in Figure 3.2B the two-way causal relationships are illustrated between pairs of components. These components are termed ECAR's (Ecosystem Components At Risk). The relationships among development actions and populations in a food web have been logically developed. Ecological risk can be quantified from the individual to ecosystem level in a rigorous and logically-consistent fashion. Note that the ECAR's can affect each other, and they are causally-related in various feedback relationships to each other. In addition, their interaction at the ecosystem level can feed back to the population and individual levels. Note also that provision has been made for evaluating the importance of cumulative effects through the multiple effects that a single development action has had on more than one type of individual (a, a') and that more than one development action can impact on the same type of individual (a) (Lane et al., 1988a,b; P. Lane and Associates Limited, 1988a,b). ECAR's cannot be affected without the logical development of causality from the individual to population to ecosystem level. Thus, ecosystem level causality can be diagrammed for all ECAR's and the other ecosystem components to which they are connected. The two-way causality can move up and down the ecological hierarchy as well as within levels, and there is also provision for cumulative impacts to be systematically studied and evaluated see Lane [1986a,b]; Wright and Lane [1986]; and Lane and Collins [1985] for descriptions of causal links and feedback in both field and laboratory marine food webs). Although this comparison has been oversimplified, it highlights the difference in the two approaches. It is suggested that an integrated approach under the rubric of environmental risk be used for the conceptual framework of offshore EEM. A similar "cluster of interlocking models" approach aimed at quantifying environmental risks is being applied for the PAH problem in Sydney Harbour (P. Lane and Associates Limited 1983b; Lane, 1987)

3.3.6 Quantitative Pitfalls and Their Solutions

Most of this report has been a discussion of this aspect of EEM conceptual framework. Many decisions about statistical methodologies and EEM design could be more efficiently resolved if there was an appropriate field data set. In Section 3.5, patterns of spatio-temporal environmental variation at sites such as Hibernia were suggested, based largely on knowledge from other regions. If EEM is to be designed to function in the presence of such variation, it is very important that the local patterns are known (Carney, 1987). Effective EEM demands a low β , yet lowering β is expensive. The design of an efficient EEM program, however, demands detailed knowledge of local environmental variation patterns. This needs to be addressed by additional field studies.

Similar field descriptive studies are also needed to support stratification schemes and seasonal sampling patterns. Sampling for some chosen variable can only usefully be stratified if its patterns of correlation with other variables are known. These are not yet documented, in any detail, for the

variables of interest on the Grand Banks. Similarly, efficient seasonal sampling patterns must be designed with reference to the seasonal cycles in the monitored variables. There is some extant knowledge of seasonal patterns at Hibernia or other east coast offshore projects, but not enough. Millard et al. (1985) have suggested that temporal correlations are unimportant in some monitoring data. If this were to be proven true at Hibernia, the EEM sampling and analytical designs could be much simplified. A baseline data set could also be used for this purpose. Data gathered appropriately for time-series and spatial-temporal correlations could also be used to fit network models of cause and effect (aspect #5).

3.3.7 Coordination with Other Components of Environmental Management

One of the major themes of Carney's (1987) review of US offshore EEM is the need to integrate all parts of the program, a need that has largely been ignored in the past. This integration is necessary for components within the program as well as for the other areas of environmental management. It would be almost impossible to relate an environmental effect to platform effluent if those effluent are not recorded (Wolfe, 1987). The end-of-pipe compliance monitoring program must, therefore, be coordinated with EEM. Similarly within the latter, the effects of oily drill cuttings on the biota are unlikely to be interpretable unless the sedimentological (cuttings distribution), chemical (oil distribution) and biological (biotic effects) data are so gathered that meaningful a posteriori comparisons of them are possible. Thus, we fully agree with Carney's (1987) suggestion that the EEM planning team should ensure that there is effective coordination and integration of all aspects of the program.

Lane et al. (1988a) have argued for establishing environmental management frameworks on a regional basis rather than that of a single development. A case has also been made for integrating environmental management components into engineering designs in the earliest planning stages so that environmental protection and even enhancement can have a rational and well planned basis. (P. Lane and Associates Limited/Washburn and Gill and Associates, 1988). Lane et al. (1988b) have argued that with better planning sustainable development can be achieved with an optimal interaction of environmental, engineering and economic concerns.

3.3.8 Logistical Realities

In the preceding parts of this report, a range of possible solutions to the quantitative problems facing offshore EEM has been outlined. Those solutions are necessarily discussed item by item but they cannot be implemented in that way. An efficient statistical design cannot lead to effective EEM if the practical execution of the program is badly handled, nor can it do so if the monitoring policy makes demands that simply cannot be fulfilled.

While the details remain to be developed, the overall conceptual framework for the design of an EEM program on the Grand Banks should include, among others: an achievable policy, suitably chosen variables to be monitored, ecological modelling to assist in the selection of those variables and to support causal

inferences, inferential tests based, in part, on the General Linear Model to determine whether there is an effect at all, an appropriate station array, and effective field methods. Altering any part of this complex will alter others; certainly determination of the "best" statistical model will depend on the policy objectives, the characteristics of the variable being monitored and the station array. Thus, development of the overall monitoring plan must be an integrated process.

The conceptual framework and the EEM design must fit with the "logistical realities". One of the best ways to achieve this fit is to make development of conceptual framework and EEM design the first priority and simultaneously conduct an inventory of the associated logistical support that will be available to implement the EEM designs. It does no good to hire an architect to design a brick apartment building when there are no bricklayers available or the building does not conform to the building code, or there are no tenants needing the accommodation.

Several of the logistical realities include:

- manpower
- resources
- cost effectiveness
- field and laboratory methods and equipment
- quantitative methods (statistics and modelling)
- data management systems
- reporting structure
- quality control/assurance
- public participation

These must be confirmed in concert with the conceptual planning.

In Chapter 5, the fundamental nature of a statistical test is described, without reference to mathematics. The limitations on the conclusions that can be drawn from such tests are emphasized, as are their requirements for particular data if the conclusions are to be valid. Specific topics discussed include the need for an explicit design phase before monitoring commences, common errors in the use of correlation and regression analyses, the absence of evidence of causality in correlation analysis, and the stringent requirements placed on any experiment if it is to demonstrate causality when analyzed by other statistical techniques. "Pseudoreplication", or the absence of valid treatment replication in the analyzed data, is considered in detail. It is concluded that the lack of replicate oil production platforms on the Grand Banks prevents the general application of most extant statistical tests to EEM data from the initial offshore oil development. Particular data sets, containing minimal temporal correlations, may be analyzable by standard inferential methods.

3.4 Recommendations

Eberhardt (1976), Cowell (1978), Green (1979, 1984), Gray (1980) Thomas et al. (1981), Phillips and Segar (1986), Segar and Stamman (1986), Carney (1987) Wolfe (1987) and many others authors have strongly urged planners of EEM to

design their programs efficiently before data collection commences. This need has been stressed throughout the present report, so neither the case for it nor the past evidence of its lack of application will be repeated here.

We conclude with four recommendations as follows:

Recommendation 1: Develop the conceptual framework for the initial offshore oil development project using the eight aspects given in this report (Section 3.3) as a guide. These aspects need to be examined to a level similar to that done here for quantitative considerations.

Recommendation 2: Evaluate the trade-offs of the various possibilities for EEM design in terms of their ecological effectiveness and costs to monitor. This is not an exercise in precision but rather "informed" decision making, which should culminate in some practical decisions for offshore EEM.

Recommendation 3: Using the results of Recommendations 1 and 2, develop the quantitative (theoretical) aspects of the EEM design including both statistical and mathematical modelling and determine the required field data.

Recommendation 4: Using the results of Recommendation 3, plan the implementation of the field studies that are needed for the quantitative aspects of the EEM design.

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5.0 APPENDIX: SOME STATISTICAL CONSIDERATIONS IN ENVIRONMENTAL EFFECTS MONITORING

A full understanding of the quantitative failings of the EEM programs reviewed in Chapter 2 requires an appreciation of several basic statistical principles. Some of these are well illustrated by examples provided by the reviewed programs. Discussion of these is delayed until the examples themselves are described in the next chapter. To aid those readers who are not well versed in statistical theory, a few other principles are discussed here as a preliminary to the reviews. It is assumed that the reader has some acquaintance with statistics, but no more than that provided by an introductory course. In consequence, the basic material common to such courses is not repeated here but is built upon in an outline of some aspects of statistics that are rarely taught to non-statisticians.

The account in this chapter is necessarily simplified, so as to clarify fundamental matters without complicating them with exceptions and marginal considerations. Furthermore, this chapter is in no way a text of statistics; it is intended only to aid the understanding of Chapter 2. EEM program design teams should incorporate professional statisticians, rather than relying on simplified accounts, such as Green's (1979) text or the present discussion.

Some authors such as Green (1979) and Wolfe (1987) have pointed out that, in EEM, statistical details are of secondary importance to the concerns of environmental management. Resolution of those concerns, however, requires that EEM be effective, and that in turn requires attention to statistical, and other, quantitative aspects of EEM.

5.1 Need for Explicit Program Design

A recurring theme, in many reviews of the application of statistics, is the need to have a clear design for the project before it commences if statistical analyses are to be used efficiently (Green, 1979; Carney, 1987). Indeed, so close is the link between design and analysis that the topics of "experimental design" and Analysis of Variance (ANOVA) become interwoven in many texts. This essential requirement for clarity of prior design cannot be overemphasized. Without such a design, the project will be, at best, inefficient and, more often, a complete failure or, perhaps worse, lead to an invalid conclusion of no detectable effect.

The requirement can be explained quite simply. Particular project objectives can only be met by particular statistical methods: a correlation analysis, for instance, cannot substitute for ANOVA. Each method requires specific types of data. This is not just the well-known need for the data to have a particular probability distribution such as the Gaussian or "normal" distribution, but also a requirement for specific groups of data to be collected. For a correlation, one must measure two variables. For a two-by-two ANOVA¹ for

¹ To test the differences of respiration rate of some animal at two temperatures and two levels of hydrocarbon contamination.

example, one must have four groups of data (Figure 5.1) if the analysis is to develop its full capability. It may also be highly desirable to have equal numbers of data points in each of those four groups (Glass et al., 1972; Green, 1979). Furthermore, if the analysis is to achieve certain quantitative objectives, such as having at least a 90% chance of detecting a 25% change in the measured variable, if such a change should occur, a certain minimum quantity of data is required. If any of these various requirements are not known and planned for before data collection begins, it is very unlikely that they will be accomplished fortuitously during the project. In consequence, the final analyses will be weak, at best, and the results obtained will be no better than those that a less expensive, but well planned, program could have achieved.

The design phase may also demonstrate that the desired objectives simply cannot be fulfilled with the available resources. Less frequently, the objectives could have been met with fewer resources than are used.

Some approaches to the design of efficient EEM programs are discussed in Chapter 3. However, the need for detailed planning leading to an explicit program design should be seen as an absolute necessity.

5.2 Concepts of Probability and Significance

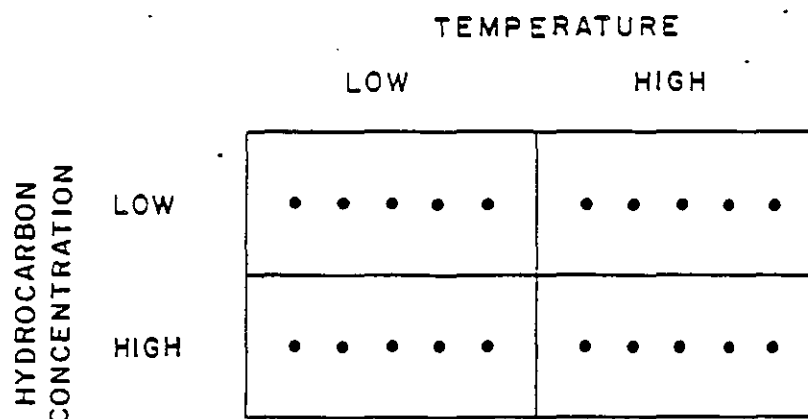
It is in the nature of statistical analyses that they do not prove anything absolutely; they can only yield the probabilities of truth and falsehood. The concept of statistical probability is straightforward, but it is incompletely understood by many non-statisticians. This readily leads to errors where real data and objectives are concerned.

5.2.1 Descriptive Versus Inferential Statistics

There are, of course, many statistical techniques that are largely or entirely descriptive, and even those techniques which are primarily designed for testing such as ANOVA, can often be usefully employed in description. None of the solely descriptive approaches can generate values of probability or significance and for some of them, such as Principal Components Analysis, there is indeed no meaningful sense in which the results can be tested. Many of these descriptive methods have a useful role in the analysis of EEM data, particularly in exploring trends and patterns. As is argued above, however, the key questions that EEM is intended to solve, such as those on which environmental management decisions will be based, can only be answered by analyses that yield more concrete, decision-oriented results. The analyses that can do so are called hypothesis testing, decision oriented or inferential statistical techniques.

5.2.2 A Simple Inferential Statistic: Student's t

The basic concepts of probability and significance in inferential statistics are best illustrated by a simple specific example: Student's t test. "Student" (Gossett, 1908) considered a situation of two theoretical data populations, for example, the body lengths of two large groups of fish. He assumed that each data population was normally distributed and that they had



• = INDIVIDUAL REPLICATE MEASUREMENT OF RESPIRATION RATE

SCHEMATIC DIAGRAM OF THE DESIGN OF A TWO-WAY
ANALYSIS OF VARIANCE (ANOVA), SHOWING REPLICATION

equal means and equal variances, that is, that the two populations were identical. He further assumed that random samples could be drawn from each population; that is, the samples could be drawn in such a way that every individual in a population had an equal chance of being in the sample. For samples of this type, he derived a test statistic, t (Figure 5.2). This is equal to zero if all of the data points in one sample happen to be equal to their opposite neighbor in the other sample and t increases as these values diverge.

"Student" showed mathematically that, if this experiment were repeated many times and his test statistic, t , calculated for each repetition, the many values of the test statistic would be distributed as a t distribution (Figure 5.2). The percentiles of that distribution are available in tabular form. They show, for instance, that for 10 degrees of freedom or 12 data points in the two samples combined, 10% of the repetitions would give a value of t greater than or equal to 1.812 and 1% of them would give a value of t of 3.169 or more.

Following from "Student's" work, it is therefore possible to draw one sample at random from each of two populations and to calculate a corresponding value for the t statistic. If the populations were known to be normally distributed, with the same means and variances, a comparison of the calculated t with the tabulated percentiles would reveal the chance, or probability, of such divergent samples having been drawn. This is trivial. If the populations were known, however, to be normally distributed with equal variances while the relationship between their means was not known, such a comparison would have some value. If there were 12 data points, and hence 10 degrees of freedom, and if the calculated value of t was 3.169, one could state that there was a 1% probability of such divergent samples having been drawn if the means were in fact equal. In statistical symbols, $\alpha = 0.01$.

5.2.3 Hypothesis Testing

To proceed further with a test, we must formulate an hypothesis to be tested. For scientific purposes, Occam's Razor² is applied and we select a "null hypothesis" designated " H_0 ," of no effect; in the case of a t -test:

H_0 : The means of the two populations are equal.

It is often supposed that α (0.01, in the above example) is the probability of the null hypothesis being true. This is not correct. α , besides being the probability of two such divergent samples being drawn at random, is the probability of being wrong if it is concluded that the null hypothesis is false. In statistical terminology, this would be a "Type I Error", the incorrect rejection of the null hypothesis. For most laboratory experiments,

² "Occam's Razor" or the "Principle of Parsimony" is a principle of logic that stems from the ideas of the medieval philosopher William of Occam. It holds that any phenomenon should be explained by the simplest mechanism that is consistent with the known facts.

$$\underline{t} = \frac{\frac{\sum X_1 / n_1 - \sum X_2 / n_2}{\sqrt{\frac{\sum X_1^2 - [(\sum X_1)^2 / n_1] + \sum X_2^2 - [(\sum X_2)^2 / n_2]}{n_1 + n_2 - 2}}}}{\left(\frac{n_1 + n_2}{n_1 n_2} \right)}$$

Where $\sum X_1$ = Sum of scores in group 1

n_1 = Number of scores in group 1

$\underline{t}_{\alpha;v}$ - The critical value of $\underline{t}$ corresponding to a given α and v degrees of freedom is available in tabular form. These are calculated by solving:

$$\alpha = \Gamma \left[\frac{1}{2} (v+1) \right] \left[\Gamma \left(\frac{1}{2} v \right) \right]^{-1} (v\pi)^{-1/2} \int_t^{\infty} (1+x^2/v)^{-(v+1)} dx$$

COMPUTATIONAL EQUATIONS FOR THE $\underline{t}$ STATISTIC AND
THE $\underline{t}$ DISTRIBUTION

To build a "test" around these ideas, a critical value of α for the acceptance or rejection of the null hypothesis must first be specified. Following R.A. Fisher, $\alpha = 0.05$ is commonly accepted as the critical value for general use, though this is quite arbitrary. Since the above t -test example yielded $\alpha = 0.01$, we would reject the null hypothesis with a 5% chance of being wrong using $\alpha = 0.05$.

If the test had yielded $\alpha > 0.05$, the converse would not be so. If we do not reject the null hypothesis, we cannot simply conclude that it is true, with a probability of being right equal to $(1 - \alpha)$. All such a "non-significant" result can show is that we have simply failed to discredit H_0 .

The choice of a critical value of α merits further discussion. In typical laboratory experiments, which culminate in a single hypothesis test and can be repeated for confirmation if they lead to anomalous conclusions, $\alpha = 0.05$ is generally useful. In major field programs, including EEM, however, there are often dozens of statistical tests performed using various combinations of species, stations, and contaminants. Taking $\alpha = 0.05$ as the critical value then leads to one test in twenty, on average, producing a significant result, even when there is no difference between the two populations. Furthermore, it is usually impractical, if not impossible, to repeat these large programs in order to check anomalous results. For such programs, and in particular for EEM, it may be necessary to use a much lower critical value of α . For example, Malins *et al.* (1984) set their critical value at $0.05/\text{number of tests}$. This approach does, however, have severe limitations. As the number of tests rises and α falls, the number of samples required to detect a real change increases. If the costs of gathering sufficient data to detect a small change when the critical value of α is 0.05 are high, as they often will be in EEM, the costs of doing so when there are, for example, 50 tests and α is 0.001, will be excessive.

5.2.4 Inferences from Hypothesis Tests

Assuming that an acceptable critical value of α has been selected and that the calculated test statistic corresponds to a probability, α , of less than this chosen value, we reject the null hypothesis, H_0 , with some probability of being wrong. In many simple situations, as in the example used here, if the null hypothesis is rejected we must accept the only possible "alternate hypothesis" or " H_1 ". Thus, if

H_0 : The means of the two populations are equal

is rejected, then we can only accept

H_1 : the means of the two populations are not equal,

as being probably true. The t -test itself, however, does not directly give any information on the probability of this H_1 being correct. It is accepted only because, on rejecting the H_0 , there is no other alternative to accepting the H_1 .

This becomes important when examining more complex systems in which there are multiple possible alternate hypotheses. For example, if

H_0 : fish in the vicinity of the production platform do not display MFO induction,

is rejected, we cannot immediately accept,

H_1 : fish in the vicinity of the production platform do display a gradient of MFO induction, decreasing away from the platform,

since it is also possible that,

H_2 : fish in the vicinity of the production platform display a gradient of MFO induction, increasing away from the platform,

or that:

H_3 : fish in the vicinity of the production platform display MFO induction, but without detectable spatial trends in the frequency of that induction.

It could be essential for environmental managers to be able to distinguish between H_1 , H_2 and H_3 , since only evidence of the former gives much indication that the production platform has caused the MFO induction. The test that led to the rejection of H_0 , however, gives no information on which of H_1 , H_2 or H_3 is correct. A separate test would, therefore, be required. This would require more data than the minimum needed to reject H_0 .³

These considerations apply if the calculated test statistic leads to a value of α of less than the chosen critical value, that is, if the test yields a significant result. In the reverse case, where a non-significant result is found, we accept the null hypothesis, H_0 , as being true but there is a certain probability, termed β , of being wrong in doing so. (A "Type II Error"; the acceptance of a false null hypothesis.⁴ Calculations of β are not so straightforward as those of α and are more normally employed in planning a program so as to minimize β , within budgetary constraints, than in an a posteriori attempt to measure the reliability of the null hypothesis.

In most laboratory experimentation, β is of limited importance once the program design stage is complete. As Eberhardt (1976, 1978), Gray et al.

³ In EEM there are temporal constraints on the collection of additional data. If H_1 is to be distinguished from H_2 and H_3 , the data to do so must usually be collected from the commencement of the program. This requires that the hypotheses be formulated and the test selected in advance. Once again, this is an argument for thorough program planning.

⁴ In many accounts β is replaced by power, where: "power" = $1 - \beta$. In this report, β is used throughout for clarity.

(1980) Millard et al. (1985) and Millard (1987a) have pointed out, β is very important in EEM. If the null hypothesis takes the usual form of "no effect" and if that hypothesis were accepted when it was in fact false, the monitored development would be permitted to continue damaging the environment. A comparison of typical EEM sample sizes with Gray et al.'s (1980) figure suggest that, for many EEM programs, β may be 50%; that is, EEM has no better than a one-in-two chance of detecting the environmental degradation. As Eberhardt (1976) pointed out, environmental management might as well be directed by tossing a coin as on the basis of such ineffective EEM. Clearly, minimizing β is vital in EEM planning.

For a given type of test, β can be reduced by any of: increasing α , increasing the number of data points in the samples, reducing the variance of the data or increasing the minimum difference in means that must be detected. None of these can be achieved easily in offshore EEM work. Increasing α naturally increases the number of false "significant" results and reduces the reliance that can be placed on those results, as discussed above. Extra sampling offshore is always expensive and often prohibitively so. The variance of the "natural" variables is not under human control and, while such methods as stratified sampling can and must be used to reduce the variance in the tested data, they can give only a limited improvement. Finally, if a 10% change in some variable is judged to be important to society, the ability of an EEM program to demonstrate that a 25% change has not occurred, with a low probability of being wrong (low β), is irrelevant. With all of these constraints, β tends to be high in EEM work. In one example, (based on the density of clams) that called for detection of a 10% change at $\alpha = 0.05$, β equalled 30% if 300 samples were taken and did not decrease to 10% until nearly 600 samples were gathered (Gray et al., 1980). Clearly, few offshore EEM programs would achieve even $\beta = 0.3$ with such data.

5.2.5 Tests for Directionality and Magnitude of Differences

The simple inferential tests discussed thus far only test for the presence or absence of a difference between two populations, not for the direction or magnitude of that difference. Many EEM programs are concerned with only one direction of environmental change, degradation. Indeed, this unidirectionality is explicit in the definition of EEM adopted in Section 1.2 (Anon., 1986). Some statistical techniques, including the t -test, permit a "one-tailed" significance test. In these, we reject as "not significant" both any difference in the samples which leads, via a small t value, to an α greater than the chosen critical value and any difference in the samples which leads to a small α while indicating an environmental improvement. The only results that are "significant" are those in which both α is small and the samples indicate environmental degradation. For EEM as defined, a unidirectional conclusion, if not simply a one-tailed test, is mandatory.

More importantly, the value of the t (or any other) test statistic corresponding to the chosen critical value of α differs between one- and two-tailed tests. For $\alpha = 0.05$ and 10 degrees of freedom, $t = 2.228$ in a two-

tailed test but $t = 1.812$ in a one-tailed test.⁵ It is clearly essential that the appropriate critical values of the test statistic are used in whichever of a one- or two-tailed test is intended. This is a special concern when using computer software packages that sometimes fail to clarify which values they employ.

In EEM, there can also be a need to test not only whether there has been environmental degradation but whether that degradation has progressed beyond some pre-selected limit. This is quite possible, but it raises two important points. Firstly, it must always be recognized that the common inference-testing statistical methods can only test for the presence of a difference, not for its magnitude. Such a test gives no information on the magnitude of the difference. Tests for differences exceeding selected magnitudes are specially designed for that purpose and only they can be used if the program objectives require such information.

Secondly, establishing that a difference exceeds some value will inevitably require more data, and hence a higher EEM budget, than merely showing that there is a difference of unknown magnitude. If the program objectives make it essential to actually measure the difference (rather than merely show that it exceeds some percentage, let alone merely that it exists) with reasonable precision, still more samples will be necessary and a larger EEM program may be required.

5.2.6 Violations of Assumptions

In the above example of Student's t -test (Section 5.2.2), it was pointed out that the relationship between calculated values of the t statistic and the percentiles of the t distribution follows mathematically from the assumptions that the two tested populations are normally distributed with the same means and variances and further that the samples are drawn from them randomly. All hypothesis testing stems from such a link between the test statistic and the percentiles of a distribution. In t -tests, the normal distributions and equality of variances (strictly "homogeneity of variances" or "homoskedasticity") are assumed, the random sampling guaranteed by correct experimental design and only the equality of means is left to be tested. Improbable values of the test statistic are then taken to be an indication of a difference in the means. Every inferential test has a list of statistical assumptions of this kind. Those of the F-test in ANOVA are the same as for the t -test, whereas some other tests, particularly the "non-parametrics", are much less demanding.

In a strict, simplistic view, if the data fail to fulfill all of the assumptions of the particular test in use, its results will be unreliable, if not wrong. In practice, all tests are able to yield acceptably reliable results when applied to data that violate some of their assumptions (i.e.,

⁵ In a two-tailed test, $t = 1.812$ corresponds to $\alpha = 0.10$. The change results because half of the possible sample pairs that would give $t > 1.812$ indicate environmental improvement, whereas only those indicating degradation are considered "significant" in a one-tailed test.

they are moderately "robust"), though few tests can tolerate gross violations. There have been extensive studies of the robustness of various tests. For the F-test of a fixed-effects ANOVA, for example, Glass et al. (1972) have shown that even extreme heterogeneity of variances has little effect on the calculated value of α , provided that the design is "balanced" (that is an equal number of replicate samples are gathered for every combination of factor values). If those sample numbers are not only balanced but also large, the same test can tolerate extreme deviations from normality (Glass et al., 1972). These tests are, however, very vulnerable to inter-correlated data (Millard et al., 1985). Such data are all too easily gathered if the sampled population is spatially or temporally structured, as most real populations are, and if the sampling is not strictly random, which is rare in field sampling.

Similar lists of permissible violations of assumptions, and of the conditions under which they are permissible, could be prepared for other tests. In practice, for a large EEM program, it is generally preferable to determine whether the expected violations will seriously distort the results of the tests to be used. For the present, it is only necessary to note that statistical tests make certain assumptions and that their violation can, but does not always, invalidate the test.

5.2.7 Field Limitations of Classical Statistics

These theoretical ideas are easily ignored when using statistics for tests of simple data sets, as in prepared classroom examples. When those same tests are applied to real data in order to solve real problems, the subtleties of the theory can be extremely important and errors in understanding can cause a project to fail.

As one example; field data are usually not normally distributed, while many statistical tests assume normality. It is, therefore, common to transform the raw data. Following transformation, some analysts test the data to see whether normality has been achieved, often using a Kolmogorov-Smirnov test. A significant result would indicate that the transformed data are probably still not normally distributed, but it would not show whether the remaining deviations from normality were large or small. The desired test may, however, be robust to small deviations from normality. Equally, a non-significant result would not be evidence of successful normalization: with small data sets Kolmogorov-Smirnov tests are unable to detect even gross departures from normality. There is a further problem with this approach. The t-test or ANOVA does not assume that the data are randomly distributed but that the populations from which they were drawn had that distribution. There are many reasons why the data and the parent populations might be differently distributed. Normality testing is useful, nevertheless, provided that it is correctly interpreted.

Similarly, some analysts seek to test differences between an experiment's control and treatment sites before applying the treatment. Once again, a significant result may be useful, but a non-significant one is not. As Hurlbert (1984) has stressed, no two sites are perfectly identical. Thus, if sufficient data are recorded from any pair of sites, they will always be detectably or "significantly" different. A "non-significant" result,

therefore, can only show that insufficient data were collected to demonstrate what was known a priori, that the sites must differ. Of course, if the question is whether or not the sites differ by more than some selected amount, it is logically possible to carry out a test, though very large quantities of data might be required.

5.3 Causality and the Limitations of Correlation and Regression Analyses

Of all of the "simple" statistical techniques, correlation and linear regression are probably the most misunderstood and abused. Since such designs as have been proposed for offshore EEM (Chapter 2) can be readily analyzed only by these techniques, some discussion of them is essential.

Correlation measures the degree of the relationship between two continuous variables^{*6} whereas regression attempts to describe that relationship without reference to its strength.

5.3.1 Correlation

Correlation coefficients, most commonly Pearson's r , have an associated probability similar to that of a t statistic. It is, therefore, possible to discuss the significance of a correlation in exactly the same manner as described for t -tests in Section 5.2. The null hypothesis must always be of the form:

H_0 : There is no relationship between the variation patterns of the two tested variables.

A significant r leads to the rejection of this hypothesis, with a probability of being wrong in so doing equal to α , and hence to an acceptance of the only possible alternate hypothesis:

H_1 : There is a relationship between the variation patterns of the two tested variables,

that is, they are "correlated".

Unlike the t statistic, Pearson's r permits a measure of the strength of the relationship as well as a measure of the probability of its existence. The proportion of the variation in one variable that can be explained by, or predicted from, the values of the other variable is equal to r^2 . This "proportion" relates to the sample data and not directly to the parent populations from which those samples were drawn. If the proportion of variation in the parent populations explained by the relationship were required, it could be estimated in the form of confidence intervals around the sample r^2 . As with some statistical elaborations mentioned in the previous

⁶ The abundance of a species and the concentration of a pollutant, for instance, but not that abundance and the presence/absence of that pollutant, the latter being a "discrete" variable

section, this may not be difficult but it does involve more than simply calculating $\bar{r}^2$.

With large data sets, $\bar{r}$ can be highly significant when $\bar{r}^2$ is very small. Even with a sample size as small as $N = 100$, $\bar{r} = 0.254$ is highly significant ($P = 0.01$) but $\bar{r}^2 = 0.0645$, that is, less than 7% of the variation can be "explained" by the relationship. Thus, correlation analysis is a very powerful technique. It can show that even very weak relationships (probably) occur. Indeed, minor errors in sampling design or measurement procedures could readily produce spurious, weak correlations between variables which would nevertheless be sufficiently strong to be "significant" if even moderate numbers of samples are collected. Consequently, correlation coefficients must always be interpreted in terms of both the probability and the strength of the correlation.

Even a strong, highly significant correlation is no evidence whatever of a causal relationship, or biological possibility, between the correlated variables. In complex systems, including ecosystems, a high proportion of all of the possible measurable variables will be inter-correlated. Some of these correlations are causal. Others result from the two measured variables being controlled by the same third, unmeasured variable. Most correlated pairs have similar, though less direct, links. A few correlations are, however, purely spurious, resulting from the chance congruence, in time and space, of variations in the two measured variables during the period of measurement. These latter are a particular problem when drawing inferences from a correlation analysis. The "chance" of such a congruence having occurred is not the "probability" of the calculated $\bar{r}^2$. That probability is the "chance" that the known relationship between the sample data reflects the existence of a relationship between the sampled parent populations, not the "chance" that the latter relationship reflects the existence of one between the two variables when measured throughout their full temporal and spatial ranges (the whole range of a species and the entire period since it was delineated by speciation, for example).

This poses a major problem for EEM work, since, by definition (Section 1.2), EEM must be able to demonstrate causality. Thus, correlation analysis may have a subsidiary place in the analysis of EEM data but it cannot provide a sufficient basis for the key question; whether or not the monitored variable has responded to the monitored development.

The lack of evidence of causality does not exhaust the dangers of uncritical interpretations of correlation coefficients. For example, consider an EEM survey transect run due east from an oil production platform at Hibernia. If the data gathered on that transect showed a strong correlation between an environmental effect and the distance from the platform, those same data would also show a correlation with distance from St. John's harbour or the oil refinery at Come-by-Chance, Newfoundland. Additional field data might readily eliminate these latter areas as sources for the pollutant effect, but the correlation itself cannot do so. As a final example, a data set containing a few pairs of observations of two completely uncorrelated variables can be made to show strong correlation by the addition of a single pair of data points with anomalously (but not necessarily erroneously) high values for each

variable. Thus, correlation analysis must always be accompanied by the careful evaluation of outliers values. In short, in the hands of an expert, correlation analysis can be a very powerful tool. In the hands of the unwary, it can be dangerously misleading.

5.3.2 Regression

Once the existence of a strong, significant relationship has been demonstrated by valid inference from a correlation analysis, it is often necessary to describe that relationship. This is a matter of measuring something, rather than demonstrating its existence (Section 5.2.5). It is the proper function of regression analysis. Within the broad area of regression, there are various ways of fitting curvilinear functions to data, with many subtle pitfalls for the uncautious user. For simplicity, however, this discussion will only point out a few aspects of linear regression. Ricker (1973) gave an excellent introduction to several forms of linear regression.

Linear regression is essentially a descriptive technique. Its function is to find the best fit of a line to a data set, and it will do so even if there is no relationship whatever between the two variables in that data set. Such a line fit to uncorrelated data would be meaningless.

Common linear regression assumes that the independent (or X) variable is measured without error and hence that all deviations of the data from the fitted line are explicable as errors in the dependent (or Y) variable. This is often approximately correct. The distance of a sampling station from an oil production platform can be measured to within a few metres over a range of a few kilometres, whereas the density of macrobenthos at that station can only be imprecisely estimated from grab samples. Other pairs of variables do not, however, provide a precisely-known independent variable (when regressing measured aromatic hydrocarbon concentrations in the sediment on measured sediment meiofauna abundance). In these situations, if the correlation is not very strong (r^2 is low), common linear regression will fit two very different lines, depending on which variable is labelled "X" and which "Y". There are potential solutions to this ambiguity, such as Ricker's (1973) "geometric mean" or GM regression. It is sufficient, at present, to note that common linear regression yields misleading and ambiguous results if neither variable can reasonably be said to be known precisely.

The straight line fitted by linear regression is the "best fit" to the data, given the assumptions of linearity and the absence of error in the independent variable. Very often, however, the best fit is not itself of principal interest. It can be supplemented by the calculation of a confidence space, within which the correct line probably lies, or by significance tests of the deviation of the fitted line from some preconceived model. These tests demand some care in the formulation of null hypotheses, but pose no other particular difficulty. Regression might show, for instance, that the concentration of aromatic hydrocarbons in the sediment decreased with distance from an offshore production platform at the rate of $1 \mu\text{g g}^{-1}$ for each kilometre. If it were known that aromatic hydrocarbon levels vary at rates of $0.5 \mu\text{g g}^{-1}$ per kilometre in the absence of production platforms, one might erect the null hypothesis:

H₀: The rate of change in sediment aromatic hydrocarbon concentrations does not exceed 0.5 $\mu\text{g g}^{-1}$ per kilometre

and test for a significantly steeper slope (a one-tailed test). Deviations of the intercept from the origin, or from any other chosen point on the axes, can also be tested.

Demonstrating that a measured slope exceeds some chosen value will almost always require larger sample sizes than are needed to establish that there is a significant correlation between the two variables. It is, therefore, essential that the entire EEM analysis be thought through to its conclusions and that the sample sizes needed to confirm those conclusions be determined before data collection begins (Section 5.1).

5.4 Replication, Controls and Causality

5.4.1 Replication and Causality in Laboratory Experiments

If correlation analysis cannot establish causality, other statistical techniques can assist in doing so. Valid inferences of cause and effect must, however, rely on a specific logical structure and an appropriate experimental design. Establishing causality can never be simply a statistical process.

As an example, consider a laboratory experiment to investigate the effect of ingested hydrocarbon on the concentration of sugars in the blood of seabirds. To refine the question, we could specify the effect of ingestion of 10g of fresh Hibernia crude oil (from a specific sample) by captive adult murre, Uria algae, on the concentration of glucose in their blood, measured 24 hours after ingestion of the oil. We then erect the null hypothesis:

H₀: Ingestion of hydrocarbons, under the specified conditions, has no effect on the blood sugar concentration, as defined, of murre.

Force-feeding the oil to a single murre would produce data on the resulting blood sugar level, but could not show an "effect" because the blood sugar level of untreated birds would not be known. Adding a single "control" bird would partially cure this problem, allowing some comparison of the two blood sugar levels. An "effect" might be detected, if there was one. However, a detected lower blood sugar concentration in the oiled bird might have many causes. The bird might, for example, cease feeding for 48 hours in response to being handled by the experimenter. It is, therefore, essential that the control bird receive identical treatment to the oiled bird in every respect except that when the oiled bird is force-fed hydrocarbons, the control is force-fed a placebo.

The results of this experiment would still not be sufficient to ascribe the detected blood sugar difference to the hydrocarbon, because we know that no two individuals of any species are identical. The control bird may be slightly more aggressive than the oiled bird and hence may obtain more food during the experimental period. If kept in separate cages, we know that one

cage will be slightly warmer, or quieter, or darker than the other and that this will affect the birds. Even if all such differences were minimized by extraordinarily careful laboratory techniques, the individual birds will still differ somewhat in liver function, with inevitable effects on blood sugar concentrations. It was precisely these sorts of uncertainties that led to the development of statistical analysis, and the best response to them in a laboratory situation remains that devised by R.A. Fisher: replication and randomization. Using a group of birds (at least four, but preferably a much larger number), and randomly dividing them into those that will receive oil and those that will receive a placebo, has several important implications for the experiment. Replication allows an analysis, such as ANOVA, to be built around the mean of individual values, thus averaging out the individual effects. The use of multiple data points also permits the estimation of variation around these means, from which the analysis can calculate confidence limits and probabilities.

The advantages of randomization are a little less obvious. Their exploration requires little mathematics. In statistically testing the above experiment, one would use the ANOVA model:

$$Y_{ij} = C + A_j + e_{ij}$$

where Y_{ij} is the measured blood sugar concentration of the i th bird in the j th group, C is the overall mean concentration, A_j is the effect on that concentration of group treatment (A_1 the effect of not receiving oil, A_2 the effect of receiving oil) and e_{ij} is the individual deviation of the i th bird from the mean of the j th group. The assumptions of ANOVA outlined in Section 5.2.6, that the data are normally distributed, homoskedastic and independent, are strictly assumptions concerning the error term, e_{ij} . In the above experiment, randomization is intended to ensure the independence of the e_{ij} values.

Non-independence would result, for instance, from all of the more active birds being oiled while the lethargic ones were used as controls. Such an obvious bias would presumably be avoided by the experimenter, but the best and, in a laboratory, the easiest way to avoid more subtle biases is to randomly allocate the experimental units (the birds) between the two treatments.⁶

There will still, of course, be some difference between the mean liver functions of the two groups of birds since, by chance, one group will have a higher proportion of active, the other, of lethargic birds. If the error term is independent, normal and homoskedastic, however, this difference is incorporated into the variance of the data. ANOVA was designed to calculate an accurate probability (not, of course, a certainty) under exactly these conditions.

⁶ If, for example, separate cages were to be used or the experiment were to take place over several days, additional randomization would be needed to prevent these factors from causing non-independence. Such complications are ignored here.

If this experiment found a difference between the treated and control birds, we could confidently ascribe that difference to the ingestion of 10g of fresh *Hibernia* crude. The conclusion of causality would depend on a combination of: a clearly stated question rendered as a specific null hypothesis that eliminates many extraneous causes and effects, inclusion in the design of a control group that allows the definition of an "effect" as a difference caused by the experiment rather than by any extrinsic factor, careful laboratory techniques that eliminate all "experimental" differences between the treatment groups except for the oil ingestion, random allocation of experimental units (birds) to the treatment groups so as to incorporate intrinsic differences between the units into an independent error term, and replication of the experimental units so as to permit both that replication and the estimation of group means and variances within the analysis. All of these steps are essential to an unequivocal inference of causality in a laboratory setting.

5.4.2 Causality in Field Experiments

All of the above logic is applicable to laboratory situations, whether the "laboratory" is a bird cage or the arable field of an agricultural research station. It is less readily applicable to many ecological field studies since: 1) concise null hypotheses are often harder to formulate, 2) the techniques needed to eliminate all differences (other than the "cause" of present concern) between the treatments applied to each treatment group are often unavailable, but particularly because 3) the random allocation of replicate experimental units to the different treatment groups is often impossible. Yet, if conventional statistical methods, such as ANOVA, are to demonstrate the causes of any detected effects, they can only do so following the same logic as employed in the laboratory.

This poses a major problem for ecological field research, but it poses a greater difficulty for the experimental investigation of artificially perturbed ecosystems, because the perturbation itself is rarely replicated, let alone randomly located. There will only be one oil production platform at *Hibernia*, for instance, and it will be positioned not only over the oil-bearing strata (which, being deep in bedrock, may be pseudo-randomly placed relative to the marine environment) but on those specific seabed sediments that can bear its weight. The development is thus neither replicated nor randomized.

If EEM at *Hibernia* is required to establish the causes of detected effects, as its definition demands (Section 1.2) this lack of replication and randomization will prevent such causality from being demonstrated by the conventional experimental and statistical approach described above.

5.4.3 The "Pseudoreplication" Question

These problems, and a series of others, were recently raised by Hurlbert (1984) paper on experimental design which has led to some controversy. Hurlbert's (1984) fundamental points, which are indeed relevant to EEM, had been raised in the environmental literature some years earlier (Eberhardt, 1976, 1978).

Hurlbert's (1984) central concern was that too many field ecologists had been analyzing or interpreting data from experiments involving unreplicated treatments as though they were replicated. He called this "pseudoreplication", which should not be confused with sample replication.

Hurlbert (1984) also elaborated on the need for both replication and randomization, making essentially the same points that are stressed in Section 5.4.3 above. He suggested: that control treatments are needed to prevent either temporal change or procedural effects from influencing the results of an experiment, that randomization is needed to remove bias, and that replication is needed to incorporate into the analysis the effects of (1) measurement error, (2) inherent variability among experimental units and (3) chance events. To these points, he added a need for interspersing of the treatments amongst the experimental units to an extent not always provided by simple randomization. This latter point is one of the difficult questions of the philosophy of statistical analysis, as Hurlbert's (1984) quotations from R.A. Fisher, W.S. Gossett and others clearly show. It will not be addressed here, although one should note that useful randomization is impossible without replication, and that for common experimental designs (that is, not Latin squares) valid replication requires randomization. Thus, separate demands for replication and randomization should be replaced with valid treatment replication. Once again, this is a repetition of one of Eberhardt's (1976) points.

Elsewhere in his paper, Hurlbert (1984) suggested a classification of types of pseudoreplication, which can be re-cast into a guide to some common errors to avoid when designing valid replication. The first point is that the collection of replicate samples does not constitute valid treatment replication. Suppose we need to test whether an oil production platform causes sediment hydrocarbon concentrations to exceed $10 \mu\text{g g}^{-1}$ at 1000m distance from the platform, and that we intend to use replication and randomization to eliminate the possible effects of natural oil seeps or other extraneous hydrocarbon sources adjacent to the platform. Collecting multiple, carefully randomized, samples 1000m from a single platform can give no information whatever on the effects of bunker oil leaking from a near-by shipwreck, because that bunker oil will affect all the samples. Replication and randomization can only fulfill their intended objective if there are replicate sites, at a random selection of oil production platforms.

The use of sample replicates as though they were treatment replicates causes at least two other problems for ANOVA. If, say, four samples were collected

⁷ R.E. Fisher was the leading "father" of statistical analysis, and particularly of biometry, the family of techniques developed for use in the biological sciences, that includes all of the inferential statistics discussed in Chapter 5. One of Fisher's greatest contributions was the notion of randomization, though he also provided much of the basis of the Analysis of Variance and many other fundamentals of modern statistics. It is, therefore, appropriate to link his name with biometry and the scientific method sensu lato that uses Occam's razor to formulate a null hypothesis which is subsequently tested by the biometrical analysis of randomly drawn data.

from each of one impacted and one control site, we would expect that (in the absence of any real effect of the impact) the samples from any one site would be more similar to each other than to those from the other site. That is, the data would be "spatially correlated", and hence non-independent. The assumption of independent e_{ij} 's would be violated and, as outlined in Section 5.2.6, ANOVA would yield a highly unreliable estimate of α .

Secondly, the variance of the treatment replicates controls the divisor of the F ratio in ANOVA and hence partly controls the final F statistic and α . Yet the variance of samples collected close together at one site is unlikely to be the same as the variance between sites some distance apart. If the sites were poorly defined, each encompassing a wide depth range and a variety of sediment types, but they were placed close together, and hence were inherently similar, then the among-samples variance in, say, clam abundance would be very much larger than the among sites variance. If, on the contrary, the sites were well chosen (all on sandy gravel at 90 m depth, for instance), then the among-sample variance could be very much the smaller. This is not a problem that can be overcome by stratification, complex station arrays or increased sampling effort. The among samples variance is, quite simply not an estimator of the among sites variance. Neither is this one of Hurlbert's (1984) disputed points. Green (1984) himself has clearly stated the inappropriateness of using sampling variance in this way.

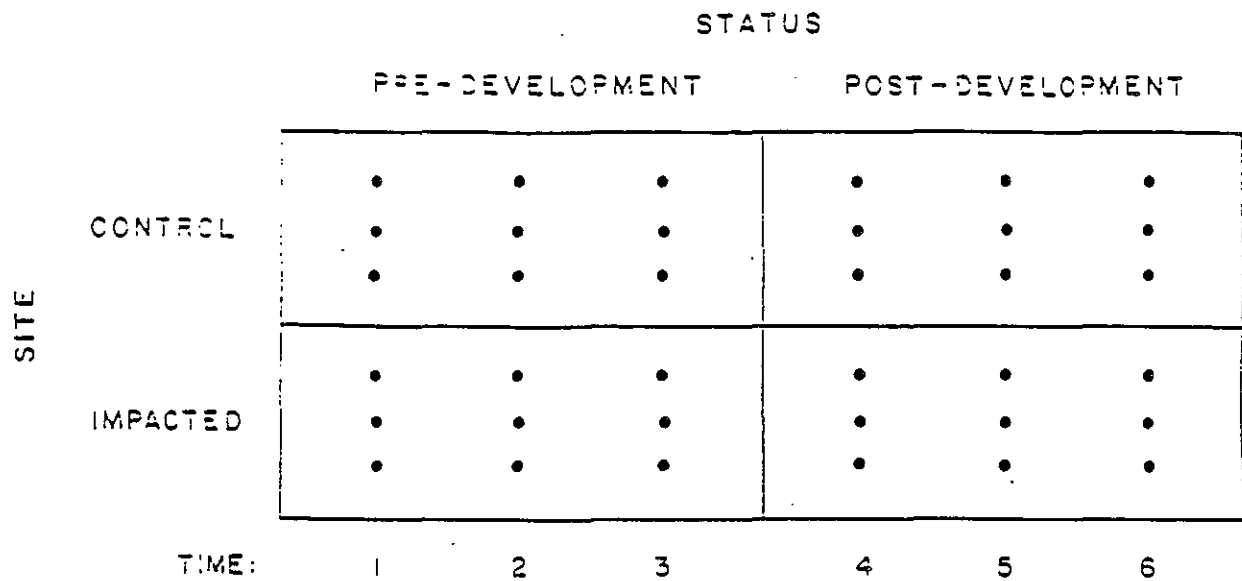
Very commonly in offshore EEM (Chapter 2), the sampling design is not stratified by depth, sediment type or other factors and the sampling variance is probably much larger than the true, but unmeasured, among sites variance. If this over-large sampling variance is used to calculate the denominator in an F ratio, it leads to a low F, an inflated calculated α and hence the false acceptance of the null hypothesis. In laboratory research, this might be no more serious than the waste of the research effort. In EEM, it would lead to the continuation of an environmentally damaging development, since "no effect" would have been detected. In short, the use of sample replicates as though there were treatment replicates is a serious error. In the absence of valid treatment replicates, obtaining appropriate estimates of the among sites variance is probably the single greatest statistical difficulty facing EEM.

Hurlbert's (1984) other category of pseudoreplication that is of present interest is his "temporal pseudoreplication". Some field experimenters have taken repetitive measurements over time at one control and one treatment site, and have then assumed these temporal replicates can be used as treatment replicates. This involves the same correlation errors as arise with multiple samples from two sites at one point in time. The spatial correlation may be somewhat reduced by temporal pseudoreplication, if there are temporal variations of similar or greater magnitude to the inter-site spatial differences. Such temporal variations will, however, often involve serial correlations (each data point being correlated with previous points from the same site). This "temporal correlation" produces a non-independent error term, e_{ij} just as spatial correlation does. As Millard et al. (1985) have recently shown, even small amounts of spatial or temporal correlation in the data lead to grievous errors in ANOVA.

As already mentioned, Hurlbert's (1984) work has encountered some opposition (Hawkins, 1986) but it has been broadly accepted by experimental ecologists. A rebuttal by Stewart-Oaten et al. (1986) addressed the subject of EEM and argued that a valid inference of causality can be drawn from ANOVA if there is only a single impacted site and a single control site. Since Stewart-Oaten et al. (1986) briefly discussed several of the points in Hurlbert's (1984) complex paper, some readers have chosen to see the former as a complete rebuttal of the latter and have used this to justify ignoring Hurlbert's (1984) ideas. It is therefore necessary to stress that Stewart-Oaten et al. (1986) agreed with Hurlbert (1984) on almost all points. They explicitly rejected Green's (1979) so-called "optimal" design. Stewart-Oaten et al. (1986) disagreed with Hurlbert's (1984) claim that an unreplicated design can do no more than measure a difference between the sampled site, for example, but they did accept that such a design cannot assign causality. Thus, Stewart-Oaten et al.'s (1986) two substantial points were: (1) that EEM (their "impact assessment") is not an experiment and hence many of Hurlbert's (1984) comments do not apply, and (2) that a valid ANOVA can be designed when there is only a single source of impact. They provided an example of such a design.

On the first of these points, their view of "impact assessment" differs from Canadian EEM (as described in Sections 1.2 and 1.3), although it is true that some kinds of EEM are not experiments. Research EEM, as required by Beanlands and Duinker (1983), however, is very specifically experimental. To have any value, it must be bound by the requirements of any other ecosystem experimentation. Furthermore, verification EEM is required by definition, to be able to establish causality, something that Stewart-Oaten et al. (1986) acknowledged that ANOVA cannot provide if given only one source of impact. Indeed, Stewart-Oaten et al.'s (1986) principal distinction between experimentation and what we would call verification EEM lies in the requirement for research experiments to provide general answers, whereas verification EEM need only deal with the particular impacted site. They then propose that randomization is needed in experiments to ensure that the treatment sites are "representative" of the generality; and hence that randomization is unnecessary in verification EEM. While we fully agree with their distinction between research and verification EEM, we do not accept that the goal of randomization is to select "representative" sites. That task is part of the function of problem bounding and null hypothesis formulation.

Their second point, that valid ANOVA is possible in the absence of replicate sources of impact, hinges on the validity of their proposed design (Figure 5.3). This requires pre- and post-development measurements at impacted and control sites. At each of these four site/status groups, a series of replicate samples are taken on each of a series of replicate times. They then propose to average the replicate samples (from each replicate time at each site; thus, incidentally, losing information) and, after suitable transformation, to subtract the control site values from the impacted site ones. They suggest that, although the original data will show temporal correlation, the control-treatment differences will not. If this suggestion is correct, these data might be valid replicates for a test of pre-development against post-development conditions (Stewart-Oaten et al., 1986).



• = SINGLE REPLICATE SAMPLE

SCHEMATIC DIAGRAM OF THE DESIGN OF A CONTROL-TREATMENT PAIRING ANALYSIS OF VARIANCE BASED ON THE PRINCIPLES OF STEWART-OATEN et al. (1986)

Just as Stewart-Oaten et al.'s (1986) objection to Hurlbert (1984) hinges on the validity of this design, so that validity hinges on the suggestion that the control-treatment differences will not be temporally correlated. This suggestion is not new. Eberhardt (1976) proposed it in his "pseudodesign", but pointed out that it demanded large sample sizes (50 to 150 samples to detect a 33% decrease at $\beta = 0.1$, if $\alpha = 0.05$, for one data set) which must not be collected at short time intervals or temporal correlation will certainly occur. Skalski and McKenzie (1982) elaborated the idea and called it "Control-Treatment Pairing" or CTP. Their work served to show that the variances of the control-treatment differences are much lower than the variances of the data, but did not address the assumption of independence of the error term. It is possible, though not proven, that short-period temporal correlations in the control-treatment differences would be small enough to disregard, but long-term trends will still occur (see Section 5.5). The CTP design lacks control data except for the pre-development period. Thus, a long-term natural change in the control-treatment differences would be indistinguishable from an effect of the impact. Stewart-Oaten et al. (1986) suggested coping with such trends either by discarding the data if a trend is detected or by measuring some index of the forces driving the change and including it in the analysis as a covariate. The first suggestion can only help with "detectable" trends, which requires judgement of which changes are natural and which artificial. Their second suggestion requires not only sufficient ecosystem knowledge to select a useful, measurable covariate but also a prediction of which trends will occur in the future so that the measurements can commence. Millard et al. (1985) have made a more useful exploration of the problem. They claimed that there was negligible temporal correlation in control-treatment difference data sets they examined. Inspection of their detailed results by Mar et al. (1985), however, found temporal correlations ranging from $r = -0.53$ to $r = 0.79$, which may not be negligible. The latter authors caution that the absence of significant correlation should be verified and importance assessed before application is undertaken.

Another rebuttal of Hurlbert (1984) is based on an assumption that a CTP ANOVA is valid. Millard et al. (1985) have suggested that, at least for some data sets, such an analysis is indeed valid. We do suggest that Millard et al.'s (1985) work could usefully be extended, since this might demonstrate the broad, though not universal, applicability of CTP ANOVA. In the meanwhile, Eberhardt's (1976) comment is still relevant:

"We have as yet not progressed very far in trying to carry the pseudodesign idea to an operationally effective stage. I am not even sure that goal is either feasible or desirable."

Valid inferences of causality, in laboratory experiments, stem from a series of steps, some logical, some practical and some statistical, that eliminate all possible causes of a measured effect except the one factor under investigation. There are several difficulties in trying to transfer this approach to EEM work, the greatest of which is the usual impossibility of

valid randomization of treatment replicates⁸. This prevents EEM from forming part of a conventional experiment, and thus removes most of the power and purpose of experimental EEM.

Furthermore, the lack of randomization and replication prevent a valid statistical assignment of measured effects to causes. While it has been suggested that a CTP design can overcome this problem, grave doubts remain.

There is, of course, the possibility that some other statistical design could suggest causality, but none have yet been proposed. Thus, barring eventual confirmation of the validity of CTP, causality cannot be distinguished statistically in EEM. By definition, however, EEM must establish causality (Section 1.2). Thus, EEM as defined is statistically impossible.

Experimental EEM must, therefore, be discarded (contra Beanlands and Duinker, 1983) as a logical impossibility. Verification EEM must rely on other logical bases for causality than those employed by laboratory experimentation and inferential statistics (sensu stricto).

5.5 Sampling in the Presence of Spatial and Temporal Variation

5.5.1 Need to Design for Natural Variation

In Section 5.2, we stressed the vital importance of minimizing β , the probability that an accepted null hypothesis is, in fact, false. A major task in minimizing β is the minimization of the variance of the analyzed data. Whether the latter is achieved by stratified sampling, by the inclusion of covariates in the analysis, by selecting certain sampling stations or by some other means, effective variance minimization demands prior knowledge of the spatio-temporal patterns of variation in the data. Furthermore, as stressed in Section 5.4, EEM designs cannot eliminate the effects on the data of extraneous causal factors; at least, they cannot do so by the conventional laboratory approach of valid randomized replication. Thus, EEM programs are vulnerable to natural spatial differences between control and impacted stations. They are equally vulnerable to natural temporal differences between the pre- and post-development phases of the project. While it will never be possible to assign causality in EEM work by statistical methods alone (Section 5.4), there are other ways in which such assignment can be achieved (Stokoe et al., 1989). If these methods are to be used, however, extraneous causal factors must be minimized; that is, the effects of natural variation patterns on the data must be reduced as far as possible. Once again, this reduction relies on prior knowledge of those variation patterns.

This aspect of offshore EEM has been almost entirely overlooked, though Carney (1987) has raised one aspect of it. Indeed, some of the monitoring designs reviewed in Chapter 2 appear to be founded on the assumption that the marine environment is as stable and homogeneous as a well-prepared green on a golf

⁸ Some small developments may be both replicated sufficiently frequently and malleable enough to be allocated pseudo-randomly to experimental units. This is not true of the Hibernia production platform.

course. Given this assumption, a design based on a few regularly-radiating transects, with stations arranged along them at logarithmic intervals and an explicit demand that the survey vessel be navigated to within a few metres of each station's theoretical position (e.g., LGL Ltd. *et al.*, 1985) would be sensible. In practice, of course, the marine environment is neither homogeneous nor stable. If the effects that the EEM program sought to measure were obvious and pronounced ("pathological": *sensu* Carney, 1987), such a "golfing green" design might still be adequate despite its inefficiency. Most of the effects that offshore EEM is required to measure are, however, neither obvious nor pronounced. Indeed, they are likely to involve the detection of a 10% change in some variable, the variance of which is 50% to 100% of its mean (a "discordant effect": *sensu* Carney, 1987). It is for the detection of such subtle, but important, effects that a prior knowledge of spatio-temporal variation patterns is essential.

The remainder of this section provides a brief review of these patterns in marine environments. It does not always address the specific patterns at Hibernia since they are poorly known and some extrapolation from other areas is unavoidable. For instance, apart from the surficial geology, no attempt has yet been made to measure small scale (less than 100 km) patterns near Hibernia.

5.5.2 Spatial Heterogeneity

Most spatial variability in the marine environment can be visualized as a hierarchy of "patches", with small ones nested within larger ones. The analogy is not perfect. In particular it is inadequate as a description of vertical structures in the water column. In general, it implies that the environment is composed of internally-homogeneous units with sharp boundaries. In practice, reality probably takes the form of areas of low clinal gradients separated by boundaries which are no more than steeper clines (the "anantoclines" and "aganoclines" of community ecology: Mills, 1969). Nevertheless, the patch hierarchy model will serve to illustrate spatial heterogeneity for present purposes.

For EEM at Hibernia, very large patches, such as the North Atlantic Ocean (*circa* 5000 km across), can be ignored. The largest relevant patch is probably the Grand Banks; a continental shelf patch about 500 km across, bordered by deep ocean to the east and south and land to the northwest, with other semi-discrete continental shelf patches to the north (Northeast Newfoundland Shelf) and west (Green and St. Pierre Banks). In bathymetry, water chemistry, water movements and biological factors, the Grand Banks have some internal homogeneity while being relatively discrete.

The important patch scales within the Grand Banks area are less obvious and will, to some extent, depend on the variables to which the "importance" is being related. From the study of many other marine environments, we would expect that bathymetry, surficial sediments and horizontal structures in the water column (for example, gyres and fronts) are all important determinants of medium-scale patches. While few specific data are available, it is clear that the waters of the southeastern Grand Banks experience warm- and cold-core gyres, with diameters of about 100 km, as well as Gulf Stream meanders of

about twice that size (Mobil Oil Canada Ltd., 1985). Many environmental variables in the Hibernia area could therefore be expected to show some spatial structure on a 100 km scale.

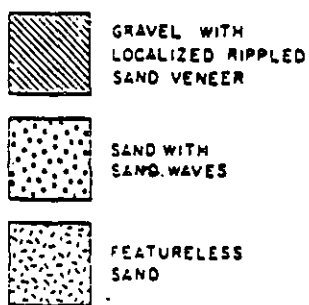
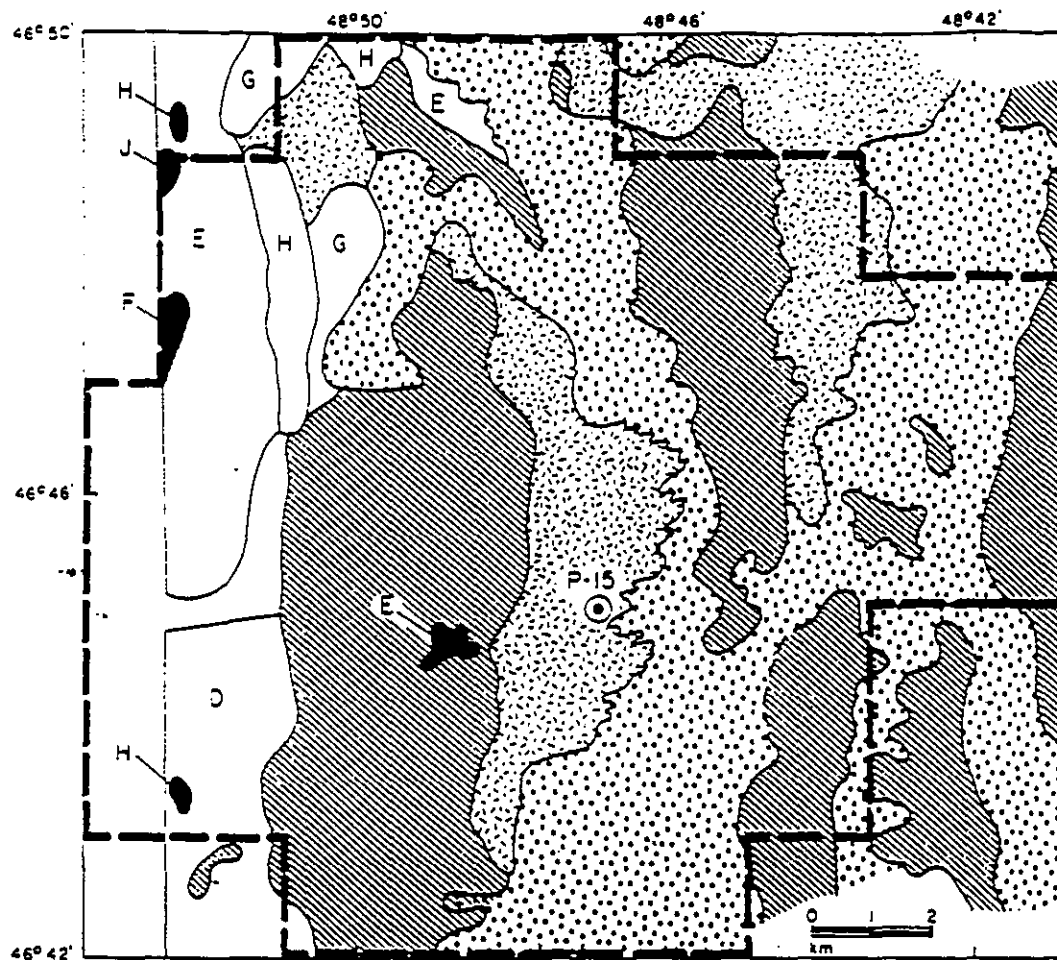
Within such patches, the seabed near Hibernia is composed of bodies of gravel with localized rippled sand veneer which are about 1 to 10 km across. These bodies are interspersed amongst patches of featureless sand, of sand waves and of some other sediment types, all of which are of the same order of size as the gravel bodies (Mobil Oil Canada Ltd., 1985: Figure 5.4). Meanwhile, satellite surveys of sea surface temperature have clearly shown that the interfaces of major currents, such as the Gulf Stream and Labrador Current, are never simple but are composed of interleaved water bodies of a whole range of sizes. There is no reason to suppose that the Hibernia area is any different. Both the benthos and interstitial chemistry will be markedly affected by grain size, while every chemical and biological process and state variable in the water column is controlled by water type and temperature. The 1 to 10 km scale spatial patches in surficial sediment and surface water will, therefore, impose the same scales on all other environmental variables in the Hibernia area.

These same considerations apply at still finer scales, with the sand veneers overlying gravel in patches smaller than 1 km, while the current interleaving at the surface is supplemented by wind-induced Langmuir Circulation cells (circa 10 m across).

Biological factors can create still other patch scales. Some of these biological patches are well known. A fish school is a small (perhaps 10 to 1000 m across) and temporally unstable patch. A scallop or surf clam bed may be similarly-sized but is more persistent in time. The clams within such a bed are not, however, uniformly distributed, rather they are grouped into dense patches and these into clusters of patches. Very little, if any, attempt has been made to measure such fine-scale structure offshore, largely because of the prodigious expense of detailed measurements in deep water. In the intertidal and shallow subtidal zones, where sampling can be both cheap and highly precise, biotic patch sizes have been shown to extend down to the spatial scale of individual organisms (Rice, 1987: Figure 5.5). Many variables that must be measured in EEM are heavily influenced by the presence of particular organisms. These may, therefore, be expected to show patchiness down to scales of millimetres or less.

There is also the additional dimension of spatial variation that cannot be viewed as "patchiness"; the vertical structure. The water column is usually wind stirred and homogeneous near the surface, with nearly homogeneous layers alternating with relatively sharp thermoclines below this. The seabed too is stratified, though only its bioturbated upper layer is of much concern to EEM. This vertical stratification, although very important and very pronounced, can usually be avoided in EEM either by sampling only one layer (above the seasonal thermocline perhaps) or by sampling the integral of all layers.

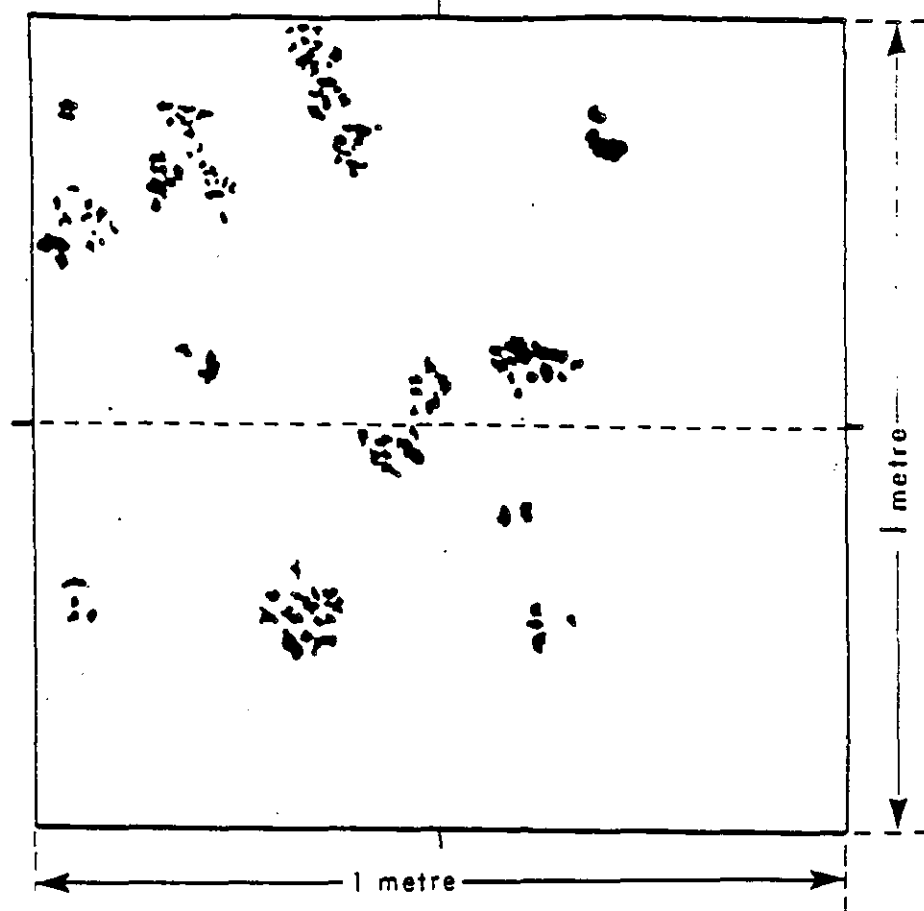
The horizontal spatial patchiness is less easily avoided. Indeed, EEM around Hibernia must be superimposed upon the spatial variation arranged on a whole spectrum of patch sizes from millimetres to hundreds of kilometres. Which



D	COARSE SAND AND FINE GRAVEL
E	GRAVEL
F	SANDY GRAVEL OR GRAVELLY SAND
G	SAND WITH GRAVEL
H	GRAVEL WITH SAND
J	BOULDER CLUSTER

SOURCE: MOBIL OIL CANADA, 1985

SURFICIAL GEOLOGY OF HIBERNIA



(DARKENED AREAS INDICATE INDIVIDUAL HOLDFAST POSITIONS ON BEDROCK)

SOURCE: RICE, 1987.

AN EXAMPLE OF VERY SMALL SCALE SPATIAL PATCHINESS
IN MARINE ENVIRONMENTS: THE SUBTIDAL MACROALGA
XIPHOPHORA GLADIATA

scales on that spectrum will show the greatest variations will depend on the particular variable being measured. Further discussion of such detail is, however, rather irrelevant since no data are available on which to base speculations. Clearly, there is an urgent need to fill this void by determining the critical patch scales for those variables of concern to EEM at Hibernia.

Some scales can probably be disregarded, however. Most sampling gears integrate over very small patch sizes. The typical 0.5 m² grabs used in offshore benthic sampling, for instance, effectively integrate over scales of less than about 0.1 m, provided that their contents are homogenized before any subsampling. Equally, very large patches can be ignored because the entire sampling program will be carried out within one patch. Thus, the problems for EEM arise with intermediate patch scales, say 100 m to 100 km.

Conventionally, small scale patchiness (say 0.1 to 10 m for offshore benthos) is treated as random error in the data. This is statistically correct, but adds to the variance of the data, hence inflates β and may obstruct the objectives of EEM. Stratified random sampling would reduce the variance, if properly planned, but would be technically difficult at these spatial scales offshore.

Rather larger scale patchiness, say 100 m to 10 km, is a much larger problem for EEM of offshore oil production since this is the scale of the station array. Patches of this size cause neighboring stations to fall into different patches, supposed control stations to be in quite different environments from their associated impacted stations and, for the 10 km scale, result in continuous spatial trends across the area studied. In the absence of treatment replication, this scale of spatial variation will always invalidate EEM based on simple analyses of a "golfing green" sampling scheme.

5.5.3 Inter-annual and Long-term Temporal Heterogeneity

With the exception of vertical structures in the water column, it is convenient to view spatial heterogeneity in terms of patch size. The same view could be adopted for temporal heterogeneity, but this is probably better seen as a pattern of, sometimes irregular, cycles of various periods with some superimposed trends and non-cyclic, random variation.

EEM at Hibernia can ignore spatial scales of the size of the Grand Banks, circa 500 km, and upwards. Equally, that EEM is confined to the approximately 20 year working life of an oil production platform and can ignore those very long temporal cycles which produce negligible change over a few decades. The longest cycle of concern to EEM, represented as almost a linear trend over 20 years, is therefore the global warming that began at the end of the "little ice age" of the eighteenth century. This trend may now be exacerbated by the "greenhouse effect". Certainly, mean global surface air temperatures have risen by more than 0.2°C in the last 20 years (Jones *et al.*, 1988) and a similar change may be expected during the period of oil production at Hibernia.

Superimposed on this long-term trend are cycles of a few decades in length, the best documented of which is the Russell Cycle of the western English Channel (Cushing, 1982). This involved major and fundamental changes in the quantity and species composition of macroplankton, and in the concentration of plant nutrients in the water. The initial changes occurred between 1925 and 1935 and they reversed between 1965 and 1980. Associated events included the decline of several fish populations (and fisheries) and the rise of others, as well as similar changes in the benthos. It appears that the predominant species, the links between them and even the basic functioning of the ecosystem altered in a cyclic fashion. The causes of these radical changes are still unclear but they appear to include slight changes in oceanic circulation, acting on complex competitive interactions among various species (Cushing, 1982).

The Russell Cycle may have had associated cycles elsewhere in the North Atlantic. Certainly the cod off West Greenland, which were numerous in the 1840's, were less abundant in 1900. Their abundance increased thereafter, largely through successful reproduction in 1917 and some subsequent years, but the population collapsed again in the 1960's under exploitation. The last really strong year class was spawned in 1963 (Cushing, 1982). The West Greenland redfish seem to have undergone a rather similar cycle (Zakharov, 1967). Both were clearly more-or-less coincident in time with the Russell Cycle and may have had related causes.

These cycles could not have been detected in Canadian waters because only the exploited species have been monitored closely enough for such changes to be detectable. Few of those are living close to their environmental tolerance limits, and it is probably species living near their limits, as the West Greenland cod and redfish do, that show marked responses to small climate cycles. There are, however, many expatriate populations in the eastern Canadian marine environment (Zurbrigg and Scott, 1972; Markle *et al.*, 1980). A sufficient change in water temperature would allow these species to become established, with consequent effects on their prey, competitors and predators.

In short, while no direct analog of the Russell Cycle has been documented in Canadian waters, radical changes in the Canadian marine ecosystem can be expected, with periodicities of decades. Any offshore EEM program must be designed to fulfill its objectives in the presence of such changes.

Within such decades-long cycles, there is lesser, but still pronounced inter-annual variability. In temperate waters above the permanent thermocline (including all of those over the continental shelves), this variability is very much smaller than that related to the annual cycle. Thus, inter-annual variation often appears as a series of annual data points. It cannot grade into shorter cycles in the same spectral manner that it grades into longer ones. Because of this almost unique discontinuity in the temporal spectrum, inter-annual variability sets a particular problem for EEM; some important environmental variables can only be measured once per year, and hence only 10 or 20 times during an EEM program. The year-class strengths of fish populations are one important example for Hibernia, but there are many others. For exploited fish populations, there are ways of calculating past year-class strengths (using Sequential Population Analysis: Ulltang, 1977) that could

generate extensive pre-development data sets. For most other annually-determined variables, pre-development data will be restricted to the few years between the proposal of the project and the commencement of development. Since many of these variables have high variance (for example, fish year-class strengths, with coefficients of variation up to 100%: Sinclair *et al.*, 1983), it is all but impossible for EEM to detect any change related to the development.

Even when this limited data problem does not arise, pronounced inter-annual variability causes the same difficulties for EEM as pronounced variability on other scales. There is the same increase in data variance, the same non-independence of residuals and the same confusion of causes in the absence of replication. The increase in variance can be serious. Fish year class strengths are variable enough: Cushing (1982) suggested that many populations show order-of-magnitude variations but that some reach two orders-of-magnitude. Some fish populations, however, can integrate reproductive success over large areas by locomotion.

Much of this inter-annual variation may be near-random, and as such would not add to the temporal correlation in the data. Some of the variation is, however, pseudo-cyclic. The best known of these "cycles" is the El Niño-Southern Oscillation (ENSO: Cushing, 1982). ENSO is driven by atmospheric changes over the southwest Pacific and its most pronounced effects are in the tropical Pacific belt. Mysak (1986) has, however, documented its dramatic effects off the west coast of Canada. These included changes in ocean currents and water temperatures, the appearance of exotic warm-water species, changes in zooplankton abundance, altered fish migration routes and fluctuations in the reproductive success of fish.

While there is, as yet, no clear evidence that ENSO drives similar changes in the northwest Atlantic, there are reasons to suppose that the global atmosphere relays its effects throughout the world. Certainly, the North Atlantic experiences pseudo-cyclic changes with periods of a few years (Cushing, 1982) that can lead to temporal correlations in annual data, whether those cycles are ENSO-driven or not.

The effects of inter-annual variation on inferences of causality are essentially the same as those of non-cyclic, unidirectional changes. These changes are common in the marine environment. One of the most dramatic was the herring epizootic in the Gulf of St. Lawrence in 1954-6. Herring were the dominant zooplankton predator in the Gulf and the principal food of cod and other top predators. The epizootic, caused by a fungal infection, killed at least half of the herring, allowed rapid cod growth for a few years while moribund herring were available, followed by reduced food and growth. There was also an increase in food for competitors such as mackerel and hence in their abundance (Tibbo and Graham, 1963). While no data were gathered for confirmation, it is entirely likely that this disease of one major species resulted in radical changes throughout the pelagic (if not also the benthic) ecosystem in the Gulf of St. Lawrence. Certainly such profound changes, caused by the linkages within the ecosystem, were documented during the Russell Cycle (Cushing, 1982).

More recently, the rocky sublittoral ecosystem along the Atlantic coast of Nova Scotia was dominated by dense populations of sea urchins (Strongylocentrotus droebachiensis). From about 1981, mass mortalities of the urchins released macroalgae from grazing pressure, leading to the replacement of urchin barrens with kelp forest (Moore and Miller, 1983). The effect on this ecosystem has been equivalent to the replacement of grassland with forest: essentially a complete transformation. These examples may be particularly dramatic but they are by no means unique. Cushing (1982) has described several major fish kills that probably parallel the Gulf of St. Lawrence herring epizootic. Cullinane and Whelan (1983) have provided another intertidal example of ecosystem change that was locally dramatic, although spatially restricted.

Thus, if EEM is to even approximately achieve its ends, it must be able to demonstrate anthropogenic change in an environment which may undergo natural, fundamental ecosystem modification during the period of monitoring. EEM design that cannot be shown to be capable of such a demonstration is inadequate. Since even an inadequate offshore EEM program will cost some millions of dollars, we suggest that those EEM designs that cannot yield useful answers in the presence of natural ecosystem modification are not worth supporting.

5.5.4 Intra-annual Temporal Heterogeneity

Within each year, the Grand Banks ecosystem varies with a series of short cyclic periods. There is the pronounced seasonal cycle (12 months), the various tidal cycles (12 hours and 14 days, particularly) which operate offshore more through the speed of tidal streams than the rise of sea level, a pseudocycle driven by storm winds (usually covering hours to days), and the very marked diel cycle (24 hours). There may be very much shorter cycles, but they pose no further problems than those already listed. Whereas the inter-annual and longer-term variations tend to cause differences between natural conditions in pre- and post-development periods of EEM the intra-annual cycles act on EEM more as a sampling difficulty within each of these periods.

Given a realistic research fleet (usually a single vessel), synoptic measurements are impossible for most monitored variables. Indeed, simply steaming the length of four 5 km radial transects and stopping at a few stations would take some hours. If the monitored variables respond to the tidal or storm (expressed as wave height) periods, it would be necessary to plan carefully the sequence in which stations are occupied. This might be better done on a Latin square design than by randomization. While perhaps few monitored variables actually respond to these cycles, the vulnerability of many species to the sampling gear will vary tidally and with surface weather conditions, leading to cycles in the variables as measured.

Both the activity and the depth distribution of many marine species change with the diel cycle. Daylight, crepuscular and nocturnal sampling will therefore yield different results. Unless all fieldwork is constrained to daylight (expensive) or a diel-cycle model is developed and all data transformed to residuals from that model (scientifically demanding), station occupancy must be randomized on the diel, as well as the tidal, cycle if the

final error term is to be independent. The detailed sampling design must, therefore, be complex.

The seasonal cycle is, however, far more demanding of EEM program designers. On the Grand Banks, it takes the form of a marked spring plankton bloom, which is variable in time, a period of relative stasis in the summer, an increased rate of change in the fall and again relative stasis in the winter. One response to such a cycle is to sample at a fixed time of year during one period of stasis (presumably the summer for logistic simplicity) and hence to avoid complexing changes in the timing of the spring bloom with inter-annual variability (e.g., Shawmont Martec Ltd. and E.G. & G Environmental Consultants, 1983). Unfortunately, the marine seasonal cycle is not so simple. If real seasonal data are examined over several years (Figure 5.6), it is easily seen that no one calendar date can provide an adequate measure of the annual level of the measured variable.

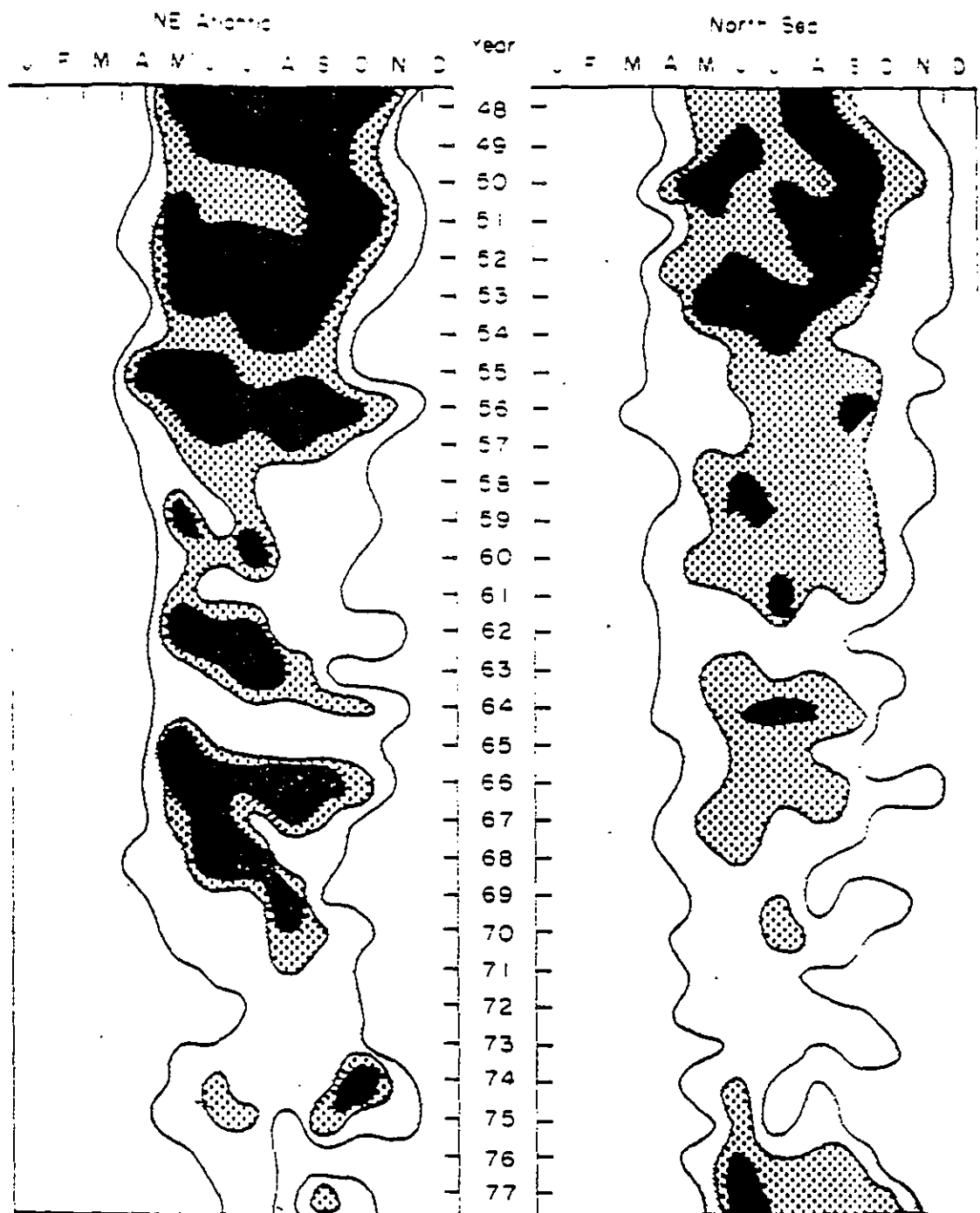
Furthermore, if the sampling is restricted to a single quasi-synoptic survey each year, the data will have the same limits on their quantity as to annually measured variables (Section 5.5.3). In short, multiple (probably monthly or rather every lunar month) surveys are needed if EEM is to yield useful answers.

5.5.5 Spatio-temporal Interactions

In the above discussions, spatial and temporal variation patterns have been treated as though they were independent. This is, of course, unrealistic. At the very least, some temporal changes will move the spatial pattern relative to geographic coordinates. Equally, some temporal changes will only occur in certain spatial patches.

Particular problems arise for EEM if a natural change is isolated in both time and space, since neither spatial nor temporal controls can assist with its elimination from the analysis. Strong spatfalls of bivalve mollusks, such as surf clams, tend to be patchy in space, even in those few years in which they occur. Once established, the resulting clam bed can persist for many years until all of the clams have died. Its presence will alter the local ecology of its vicinity. Clearly, no statistical analysis can detect the effects of an oil production platform, if part way through the data collection program, part of the station array experiences this type of perturbation. In the absence of replicate platforms, however, that is exactly the task demanded of EEM.

The nature and degree of natural spatial and temporal variation in marine environments is described. Offshore EEM programs must gather data in the presence of this variation. To be effective, they must be able to identify the effects of the monitored development from such data.



SOURCE: CUSHING, 1983

AN EXAMPLE OF INTER-ANNUAL VARIATION OF SEASONAL CYCLES IN MARINE ENVIRONMENTS: PRODUCTION OF ZOOPLANKTON BIOMASS IN THE NORTH EAST ATLANTIC AND NORTH SEA

Synopsis for
ENVIRONMENTAL EFFECTS MONITORING:
PITFALLS AND POSSIBILITIES
IN RELATION TO OFFSHORE
OIL DEVELOPMENT

Project E183

1989b

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SYNOPSIS

Canada's first offshore oil production platform will be installed on the Grand Banks, off St. John's, Newfoundland, within a few years. The developers of offshore oil will be required to conduct an Environmental Effects Monitoring (EEM) program in co-operation with the Canadian Newfoundland Offshore Petroleum Board, Environment Canada and other government agencies. Some of the conceptual framework for such EEM programs is available, and it provides a basis for decisions on what to monitor and, in practice, how to monitor. The quantitative aspects of offshore EEM, however, have received less attention.

The objectives of this report are to review past approaches to EEM of offshore oil operations, to identify the pitfalls of these approaches with particular reference to their quantitative aspects, and to suggest possible directions for the initial EEM program.

1.0 A CONCEPTUAL FRAMEWORK FOR EEM

To design a conceptual framework for EEM for offshore development, three basic points must be emphasized: (1) the biological hierarchy; (2) the nature of environmental change; and (3) conceptualization of causality and interrelationship at the ecosystem level. First, the causal relationships linking the development activities (chronic oil discharges, shipping, etc.) to potential effects in the biota must be viewed in a hierarchical way. Biological systems by their nature are hierarchically arranged and this influences all causal relationships among the biota and other aspects of the environment.

If an effect is to be observed for any particular level of the hierarchy (for example, the population level), then the appropriate field, laboratory and quantitative methodologies must be in place to detect the change. In addition, the causality from the discharge of the pollutant (or other activity) into the environment to the effect must be understood and at least plausible, if all causal links are not fully documented. Generally, as one proceeds from the lower levels of the biological hierarchy to the higher ecosystem level, cause and effect become more difficult to identify and document, and non-specific system response are observed. This non-specific system response may be causally related to many perturbations both natural and anthropogenic ones. Furthermore, the causality is more difficult to document for chronic and routine pollutant discharges than for major oil spills.

The concept of EEM centers on the detection of change. Change in and of itself is universal in ecological systems. To document change, there must be a baseline and a time (or set of times) in the future when a change is to be observed. Often, absolute change may not be as important as pattern of change. Usually some sort of inferential statistical analyses must be invoked to determine the probability of whether the change is "real".

Change is a relative concept, since the magnitude of change must always be related to a particular space and time framework (P. Lane and Associates, 1985). In a biological hierarchy, change occurs at different temporal and spatial scales. It is also not enough to demonstrate change in undertaking environmental effects monitoring, it must also be shown that the observed change is causally related to the development activities.

With an offshore development such as Hibernia, various spatial scales are also important. Three areas can expect to be impacted: (1) drilling area, (2) land-based facilities, and (3) shipping routes. Because of the differences in development activities in each of these areas, recommended sampling methods and quantitative methodologies for EEM are also different.

If the appropriate spatial scale is not selected for a particular type of development activity and its potential effects, then a biased conclusion may result concerning the importance of the observed changes and they may not appear to be real effects causally related to the development. For example, in sampling changes in benthic community diversity around the drill site, a scale interval of 5 m may be too small and a scale of 5 km would be too large. Measures of change would be very different at the two scales.

The way causality and interrelationship are conceptualized is critical to the success of any environmental effects monitoring programme. To date, much of the management of complex systems has failed because of the failure to understand interconnection (P. Lane and Associates, 1985; Lane, 1986).

A good conceptual framework should include, or provide the rationale for the following:

- 1) adequate scoping with explicit objectives and a clear distinction between the roles of science and policy.
- 2) a way to select variables that would span the biological hierarchy while being meaningful to monitor from an environmental management perspective.
- 3) adequate bounding of space and time relationships of the development and the environment. Central to this is the identification of environmental change.
- 4) an adaptive approach that will encompass as wide a range of human activities, both routine and accidental, related to the development as resources will permit.
- 5) a methodology to frame the understanding of cause and effect emanating from a development activity, through the biological hierarchy to the ecosystem. This is best accomplished through a cluster of models, perhaps centered on the concept of environmental risk.
- 6) an awareness of the quantitative pitfalls and their solutions culminating in appropriate sampling designs and data analysis and interpretation.

- 7) an awareness of how the results of EEM can be made most useful for environmental management including the integration of EEM with other forms of monitoring and assessment, public participation, protection planning, contingency planning and, in particular, areawide assessment and/or cumulative effects assessment and management, and policy decision framework. This should include a clear definition and understanding of significant environmental change.
- 8) attention to the various logistical realities:
- manpower
 - field and laboratory methods and equipment
 - quantitative methods (statistics and modelling)
 - reporting structure (data management systems)
 - budget.

1.1 Related Literature

There is a large body of recent literature on environmental effects monitoring (EEM) which will, collectively, form the scientific foundation of the first offshore oil development monitoring plan. Conover (1985) and Barchard and Moores (1988) have outlined two views of the nature and role of EEM, while Beanlands and Duinker (1983) and Munro et al. (1986) have each attempted to place EEM within the wider context of environmental impact assessment and environmental management. The above reports have all been broadly conceptual, and they are not fully in mutual agreement.

The design of a practical EEM program requires more detailed advice in at least three areas: the qualitative question of what to monitor, the practical question of how to measure it, and the quantitative questions of when, where and how often to measure it. What to measure has been addressed by a great number of studies, summarized most recently, but with no particular focus on the EEM of offshore oil production, by Maritime Testing (1985) Ltd. (1987). How to measure the chosen variables is the subject of much practical experience but has received less attention in the literature. Environment Canada (1987) does, however, provide a detailed description of methods appropriate to one Canadian marine environment, the Arctic.

The remaining, quantitative question forms the focus of the present report. This question has been neglected in the past, despite much of the necessary statistical background being clearly enunciated a decade ago by Eberhardt (1976). His comments have recently been taken up again by Millard et al. (1985), Millard and Lettenmaier (1986) and Millard (1987a,b). These recent papers have been largely theoretical, however, and directed to EEM of power plants, not of offshore oil platforms. The only published work directly comparable to the present report is Carney's (1987) recent and perceptive review of the quantitative and design aspects of EEM of offshore oil installations in U.S. waters. Many of Carney's (1987) points are discussed herein, and this report should be read in conjunction with Carney's (1987) work.

Environmental management procedures must include programs for the routine monitoring of many variables, from the levels of various anthropogenic inputs to the environment to the impacts of changes in that environment on human affairs. This wide variety of variables to be monitored leads to an equally wide range of types of monitoring programs.

Within this range of activities, "Environmental Effects Monitoring" (EEM) is intended to measure the effects of particular anthropogenic inputs on the environment, and more specifically on the biotic or, at most, the bio-physical environment. For this purpose, an "effect" is usually a change in some environmental variable, measured either in space or time, that can be attributed to an anthropogenic causative factor. EEM has therefore been defined by Environment Canada (Amon., 1986) as:

"A program of observations for the purpose of determining whether the presence, or change in the incidence, of a factor(s), has adversely affected critical biological processes, or the physical and/or chemical nature of a system. (The effect so detected should be attributable to a specific action, agent or process of man)."

This definition is followed in the present report with some latitude appropriate to a theoretical (rather than operational or regulatory) context, of the stress on "adverse" effects and "critical" processes.

EEM, as defined, includes neither Surveillance Monitoring, which lacks regard for particular human actions, nor Compliance Monitoring, which is concerned only with "factors" and not with their effects. An overall monitoring program must include other types of monitoring as well as EEM, but only the latter is considered in this report. EEM is concerned only with effects on the natural environment and not with effects on human affairs. EEM as defined above includes monitoring designed to detect the reduction of a resource, but not for the detection of the associated social and economic impacts.

A further distinction may usefully be drawn between EEM and "biological monitoring". While the latter term is sometimes (Dalby, 1979) used to include the monitoring of biological variables (often, but not always, an aspect of EEM), biological monitoring is more commonly the use of biological systems to monitor chemical contaminant levels, for example, body burden measurements (Baker, 1976). This approach has been extensively used, with considerable success, but it is not usually EEM. Biological monitoring programs are not generally designed to detect the effects of the chemical distributions that they measure and, in consequence, they do not detect such effects.

The power of EEM, not to mention the quantitative aspects of its use, depends, however, upon on the chosen definition of an "effect".

In this report, we largely concentrate on the hierarchical view of environmental effect for two reasons. First, the power and purpose of EEM

stem from its (potential) ability to measure changes in variables of ultimate interest to mankind. Measurements of small scale movements of fish are unlikely to be of much interest to the general public, whereas the failure of reproduction of an exploited population would be of great economic importance. Eventually, EEM must be able to address the population and ecosystem levels of the hierarchy.

The second reason for the present emphasis on higher-level effects is that they pose the major quantitative problem for EEM. Carney (1987) has pointed out that the planners of most EEM programs have implicitly assumed that the effects they sought to measure would be "pathological"; that is, something would be observed that could only be an effect of the anthropogenic stress of immediate concern. Many environmental effects are, however, "discordant" (sansu Carney, 1987), which demands a quantitative, and specifically a statistical, approach and so that is the type of EEM of principal concern here.

1.2 Types of EEM

Even within the definition used here, EEM can have any one or more of several distinct objectives. These include:

- a: to provide early warning of undesirable change,
- b: to improve environmental understanding, especially of cause-effect pathways,
- c: to verify impact predictions,
- d: to check effectiveness of mitigation measures, and
- e: to satisfy regulations and public concerns.

Although these objectives are similar, they require quite different EEM approaches and hence three, more or less distinct, types of EEM can be recognized.

Early warning (objective a) is essentially predictive and anticipatory. It must involve frequent, rapid measurements of simple variables and some form of model which predicts what ultimate effects will arise if present trends in contaminant discharge, or other input factors, continue.

A quite different approach to EEM is implicit in objective (b). This approach treats the monitored development as a scientific experiment in environmental perturbation and EEM as its measurement and analytical phases. Monitoring for this objective is, therefore, a purely "scientific" process and should meet the normal standards for scientific publication.

Objectives (c) and (d), verifying the effectiveness of mitigation and the accuracy of the predicted impacts, involve some of the attributes of each of experimental and early warning EEM. In consequence, the field and analytical aspects of such "verification" EEM will, in part, be some combination of those already discussed. Verification EEM is, however, primarily a management, rather than research, task. As such, some of the scientific rigor required in experimental EEM can be relaxed for verification EEM.

Finally, objective (e), EEM to satisfy regulations and public concerns, combines scientific and non-scientific elements. Clearly, there is a real need for all monitoring activities to fit into a larger political and legal context and, where the regulations are themselves framed around one of the preceding objectives, satisfying the regulations will require only minor modifications to other EEM designs.

1.3 Distinction Between Monitoring and Research

One final clarification of the scope of EEM is essential. In this report, the term "environmental effects study" will be used to encompass all scientific (in the broadest sense) activities designed to elucidate the effects on the environment of an anthropogenic stress. These studies fall into one of two broad classes: "environmental effects research" and EEM. The difference starts as a matter of purpose and intent, but produces fundamentally different types of results.

The distinction between environmental effects research and EEM is somewhat blurred. This distinction is, nevertheless, important in the design of effects studies. It has been recognized in the monitoring literature for several years (Thomas et al., 1981). Recognition of this difference between monitoring and research has an important corollary; monitoring is a facet of environmental management, not of environmental science. This position is widely accepted in the monitoring literature (Hecky et al., 1984; Larkin, 1984; Munro et al., 1986; Ter Keurs and Meelis, 1986; Wolfe, 1987), but, as discussed in Section 1.3 below, an opposite view is becoming established in Canada.

Coincident with these largely theoretical reviews, Beanlands and Duinker (1983) prepared a review of the state of environmental impact assessment in Canada, specifically as a framework of recommendations for future assessment procedures. Among many valuable suggestions, they included calls for more quantitative approaches, for more pilot-scale perturbation experiments and particularly for the inclusion of thorough monitoring programs as integral parts of the assessment process. They also stated:

"There is a growing conviction that the project must indeed be considered in an experimental context in which post-project monitoring is required to test the hypotheses (the impact predictions)." (Beanlands and Duinker, 1983, p. 7)

They expanded on this comment elsewhere (their pp. 58-60) and built upon it the idea that impact predictions should take the form of testable, or falsifiable, hypotheses (their pp. 85-87), following the Popperian¹ view of

¹ Karl Popper is one of the foremost philosophers of science. He is most famous for linking the Scientific Method with the idea of a "falsifiable hypothesis". In oversimplified summary, he suggested that no hypothesis can be proven by science; an hypothesis can only be disproven.

the "Scientific Method" (Popper, 1959). Thus, Beanlands and Duinker (1983) produced a valuable, multifaceted framework for impact assessment, but within that framework they took Rosenberg *et al.*'s (1981) extreme view, that assessment be structured as a research activity incorporating the "scientific method" (*sensu lato*), and narrowed it to the point that an assessment would be a strict, hypothesis-testing experiment of holistic ecosystem responses to perturbation, incorporating the "Scientific Method" *sensu stricto*.

Beanlands and Duinker's (1983) recommendations, including their stress on the testing of falsifiable hypotheses, however, are beginning to form the basis for much Canadian environmental impact assessment work. At least one recent set of Federal Environmental Assessment Review Office (FEARO) guidelines has been founded on those recommendations (FEARO, 1987). Equally, a very large effort has been devoted to the Beaufort Environmental Monitoring Project (BEMP), which is essentially an exercise in hypothesis formulation with the implicit assumption that the hypotheses are testable (LGL Ltd. *et al.*, 1985; Indian and Northern Affairs Canada and Environment Canada, 1985). Finally, Conover (1985), reporting the views expressed at a suite of Environment Canada workshops, defined EEM as essentially a research activity. This is incorrect. Thus, there is a danger in attempting to equate two very different types of activity, namely doing science and conducting EIA's.

2.0 ENVIRONMENTAL EFFECTS MONITORING OFFSHORE: RECENT EXPERIENCE

The concept of EEM is barely ten years old, whereas the working life of an offshore oil production platform is typically twice as long. In consequence, no EEM of offshore oil production could have been completed, in the sense of extending from pre-development, baseline measurements, through the construction and production phases, to the monitoring of long-term contaminants after the platform is closed down.

There have been several effects studies around offshore exploration wells. Some of these have been completed, in the sense of extending through the working life of the well, since that is a brief period. Most of this work, however, has involved short-term, highly specific research (for example, NORDCO Ltd., 1983; Yunker *et al.* 1985). Only the Mid-Atlantic Block 684 Study and the Georges Bank Monitoring Program have approached EEM, as it is understood here (Carney, 1987).

A scientific hypothesis can then be distinguished from an unscientific hypothesis, because the former can be disproven or falsified while the latter cannot be. The "Scientific Method" is an iterative sequential process of inductive thinking, erecting a falsifiable hypothesis, subjecting it to a critical test, and returning to more thinking. Many philosophers, scientists and others consider this the only true Scientific Method. In this report, it is termed the "Scientific Method" or associated with Popper's name.

The first, and one of the most important, conclusions of this review is, therefore, that the EEM proposed for the initial offshore oil development on the Grand Banks is largely novel. While past experience in many fields may aid the design of the EEM program, it must be a new design and not a small modification of a previous approach.

Since there have been no programs for EEM of offshore oil production, taking "EEM" as it is defined in Section 1.2, the present chapter cannot be a conventional review. Instead it is a broad overview of four groups of studies: (1) the three reports of environmental effects measurements around Gulf of Mexico oil production installations, (2) the environmental effects research program in the northern North Sea, (3) recent approaches to EEM off the US coasts outside the Gulf, and (4) existing Canadian plans for EEM of oil production at Hibernia and in the Beaufort Sea.

2.1 Gulf of Mexico Experience

Three major studies were carried out in the Gulf of Mexico in the 1970's: the Offshore Ecology Investigation, the Central Gulf Platform Study, and the Buccaneer Gas and Oil Field Study. These were all a posteriori studies of the environmental effects of prolonged oil production.

2.1.1 Offshore Ecology Investigation

Environmental effects studies in the Gulf of Mexico began with the Offshore Ecology Investigation (OEI; field seasons 1972-4). This was intended to examine the effects of offshore oil production in the "worst case" region: the Louisiana coast. OEI used a complex array of stations, comprising several "ambient stations" in areas assumed to be exposed to the effects of petroleum production, nearby "control stations" in areas assumed not to be exposed to those effects, five offshore transects and a scatter of regional stations. Upon this station array, the various collaborators performed their own research projects with, apparently, little coordination (Carney, 1987).

Carney (1987) has criticized the Offshore Ecology Investigation for:

- 1) not gathering sufficient prior information before designing the elaborate field effort, which led to a failure to allow for natural variation in the study design,
- 2) not clearly defining the central goals and also failing to produce, a priori, operational definitions of such items of concern as "ecosystem health",
- 3) a lack of coordination between the various parts of the OEI, leading to both a failure to study some numerically-dominant organisms and an inability to relate the findings of the various parts to one another, particularly the relation of the biological results to abiotic variables including ambient hydrocarbon concentrations.

- 4) a station array in which the "controls" were merely assumed, with no empirical evidence, to be unaffected by oil production and also in which there was insufficient replicate sampling to establish anything, given the expected natural variation,
- 5) pooling samples or data, thus hiding the true variance and giving a false impression that the results were meaningful,
- 6) not using quantitative analyses other than purely descriptive methods, such as classification (cluster analysis), and
- 7) presenting opinions based on these dubious results as though they were firm conclusions.

Carney (1987) acknowledged that the overall conclusion of OEI was that natural variation had prevented a conclusive statement of the effects of oil production, but noted that OEI's results have been taken as evidence of "no effect" rather than "no proven effect" by other workers. Clearly, this is a particularly dangerous outcome for EEM which, in statistical terms, stems from β being too high.

2.1.2 Central Gulf Platform Study

OEI was succeeded by the Central Gulf Platform Study (CGPS) of 1978-9. This was largely a study of the fates and effects of chemical pollution from offshore production platforms, with only limited attention given to faunal analyses.

The major improvement of CGPS over OEI was in the later program's station array.

Carney (1987) recognized that CGPS was an improvement over OEI, but still found several points to criticize. These can be summarized as:

- 1) the goals of the CPGS were still poorly defined, with no operative definition of the "impact" that the study sought to detect and no prior hypotheses to be tested,
- 2) the station array could have been well suited to a final analysis by ANOVA, but the sample replication was discarded by pooling and the analysts proceeded with an array of less powerful technique such as subjective interpretation of data tables, classification and the correlation of biological and other environmental variables,
- 3) such inferential statistics as were used were poorly handled in several ways, with particularly severe errors in the use of correlation analysis, and
- 4) the stated conclusions were not supported by the weak analyses used, while there was a tendency to express opinion as fact.

- 5) CGPS worked stations no closer than 500 m from the monitored production platforms, whereas it is now clear that only very subtle environmental effects will occur that far away (Davies et al., 1984), so the study was always unlikely to detect anything,
- 6) most of the analytical techniques used were not suited to the goal of the study: classification is a purely descriptive process which could only detect the effects of pollution if those effects constituted large changes in the community, whereas correlations of biological to abiotic environmental variables yield no information on the effects of pollution unless the pollutant level is the environmental variable analyzed, and
- 7) the remaining analytic technique employed, subjective interpretation of the data, is a valuable first step, but because it is subjective it cannot be used to reject null hypotheses or to show causality.

Despite these criticisms, CGPS had some notable design strengths, particularly the use of replicated study sites (the multiple production platforms), the inclusion of control sites (albeit imperfectly selected) and the use of crossed transects at each study site. All of these features have value.

2.1.3 Buccaneer Gas and Oil Field Study

The third major environmental effects study in the Gulf of Mexico, the Buccaneer Gas and Oil Field Study, overlapped chronologically with CGPS (survey seasons 1976-7: Carney, 1987). The Buccaneer Field, off Texas, was chosen for this work because its relative isolation should have allowed near-pristine control areas to be included in the station array. The benthos sampling effort for this study also relied on radial transects extending from the production platform.

The Buccaneer Study repeated several of the errors of OEI and CGPS, but Carney (1987) also identified the following problems:

- 1) the coordination of the various study components was poor; the data from each component were plotted as contour maps, and compared as the primary integration of the various data types,
- 2) such sample replication as was included in the design was discarded by pooling,
- 3) the data were analyzed with a combination of classification and ordination techniques and subjective interpretation of plots, which are subject to many criticisms discussed elsewhere in this report, and
- 4) when the Study showed a decrease in benthos abundance within 100 m of the production platform, but no significant change in diversity, the authors concluded that this was evidence for the effect of scour, not chronic pollution. This was presumably based on the assumption that

pollution, is not a "natural" stress such as scour, but will lower diversity. Their data, however, were insufficient to distinguish between these causes.

The following additional criticisms of the Buccaneer Study can be made:

- 5) the deliberate alignment of the transects imposed a subjective element on the data, removing some generality from any conclusions that might have been drawn, and
- 6) the use of asymmetric radial, rather than diametric, transects would have complicated the elimination of monotonic spatial trends from the data, had analysis proceeded that far.

OEI, CGPS and the Buccaneer Study did not detect any important effects of oil production, but it is felt that, because of design faults, they could not detect any but the most obvious of environmental damage. Conclusions of "no detectable effect" are still being cited as evidence of "no effect" (Somerville et al., 1987) and may lead to the perpetuation of errors in EEM designs, environmental management decisions, and perceptions of the environmental risk of offshore oil operations.

2.2 North Sea Experience

Offshore oil and gas production has proceeded in the North Sea for many years. Almost all of the oil fields lie in the northern part of the area, and hence in the United Kingdom and Norwegian sectors. The Norwegians have required some mandatory routine monitoring work, but this has only been compliance monitoring (LGL Ltd. et al., 1985). In the United Kingdom studies of the environmental effects ("biological effects" in the British terminology) of oil production tend to be an environmental calibration of the end-of-pipe chemical compliance monitoring. In consequence, although a wide variety of pollution studies and environmental effects research projects have been carried out in the North Sea, long-term, large-scale, routine EEM programs have not been attempted. LGL Ltd. et al. (1985) have presented a detailed review of non-quantitative aspects of the North Sea environmental effects programs studies.

The primary cause of the North Sea emphasis on effects studies, rather than effects monitoring per se, was the realization that biological effects monitoring of offshore oil installations is simply not practical at present (Dr. A.D. McIntyre, pers. comm.). The need for effects monitoring was acknowledged as early as 1975 by the ICES Working Group on Pollution Baseline and Monitoring Studies in the Oslo Commission and ICNAF Areas (McIntyre et al., 1978). This led to a major report on the feasibility of such monitoring (McIntyre et al., 1978) and, in 1979, to a workshop at which the existing knowledge base was reviewed (McIntyre and Pearce, 1980). The latter produced a realistic appraisal of the problems facing effects monitoring programs (Gray et al., 1980) and precipitated the research which McIntyre et al. (1978) had called for as a prelude to EEM. That research has been ongoing, but it has not led to the rapid introduction of effects monitoring programs (McIntyre, 1984). Indeed, there has recently been a

subtle, but highly meaningful, change in terminology from "biological effects monitoring" to "biological effects studies" (Dr. A.D. McIntyre, pers. comm.).

The North Sea work on the environmental effects of oil production has been structured as a series of partially well-planned research projects, rather than as EEM per se. In the process, it has built the only substantial body of knowledge concerning the effects of offshore oil production. The studies have not, however, used quantitative analytical methods and, in particular, have not used inferential statistics. Their conclusions are, therefore, somewhat subjective and remain untested by more rigorous techniques. Conclusions regarding offshore oil installations should not be accepted unequivocally on the basis of any of the non-quantitative North Sea work.

There are two major lessons for quantitative design of Canadian EEM programs. First, the North Sea scientists have shown that environmental effects studies can be efficiently designed and executed. More importantly, they have shown that effective EEM must be preceded by extensive supporting research. In the opinion of these researchers to date, ten years of research in the North Sea have still proven insufficient. While Canada might hope to build on the North Sea results, their experience suggests that EEM for offshore Newfoundland cannot be both fully operational and fully efficient without considerable preparatory work.

More specifically, the North Sea work has raised three useful suggestions: that long-term studies are essential (Gray, 1980), that stratified random sampling could lead to greater efficiency (Gray, 1980; Gray et al. 1980) and that grid sampling may be preferable to radial transects for sampling around offshore platforms (Hartley, 1982).

2.3 Other Monitoring Programs off the Coasts of the United States

Three recent offshore EEM Programs have built upon the experience gained in the Gulf of Mexico and the North Sea. These are the Mid-Atlantic Block 684 Study, the only comprehensive program for EEM of an offshore exploration well, the Georges Bank Monitoring Program, a first attempt at regional-scale integrated monitoring of both exploration and production, and the California Monitoring Program which is intended to be an integrated study of multiple production platforms. These three programs represent the nearest approaches to date to the sort of offshore EEM intended for Hibernia.

2.3.1 Mid-Atlantic Block 684 Study

This study of an exploration well off Atlantic City, New Jersey was partly coincident in time with the Gulf of Mexico studies discussed in Section 2.1 (field seasons 1978-9). Compared to those Gulf studies, the Block 684 work was well planned. In particular, it had excellent coordination between its various parts and it incorporated an explicit definition of the "impacts" sought (Carney, 1987).

Nevertheless, this study was still flawed. The fundamental problem lay with the design of the station array. This was an elegant refinement of the "bullseye" pattern used in the Central Gulf Platform Study.

Since this design was, in effect, the ultimate development of the "golfing green" approach, its limitations are discussed in detail. If the marine environment were as homogeneous as a green, if the relevant pollutants did diffuse outward from the platform and if those pollutants had a linear dose/response relationship, there might be a logarithmic decline in environmental effects with distance from the platform. In such a case, and if the objective of monitoring was to describe the parameters of the decline with distance, a station array involving logarithmic station intervals on radial transects would be optimal.

The marine environment is, however, spatially heterogeneous. The quantitative effect from a given level of pollutant will, therefore, depend on what kind of patch the station is set in. In addition, most pollutants will be spread by advective rather than diffusive processes. The patchiness of advection, caused by water column gyres and similar features, will lead to a patchiness in the pollutant loading to the already patchy natural environment. Furthermore, the response of the environment to pollutants is not linear; environmental effects will often appear when the pollutant loading exceeds some critical value.

This is not, however, the only flaw of the Block 684 Study station array. One of the conditions for that array to be optimal is that its objective is to describe the variation of the effect with distance from the platform. While this is an interesting research question, and may indeed have been the objective at Block 684, it is an inappropriate focus for most EEM. As is repeatedly argued in this report, EEM requires not description but decision-oriented inference. For "discordant" effects, that inference requires an hypothesis test.

Criticism of the Mid-Atlantic Block 684 Study is lengthy, and there is no doubt that the study was flawed. In relative terms, however, it was much better designed and executed than any of the contemporary Gulf of Mexico studies. In part, it is this better design that permits detailed criticism, which is not possible for much of the Gulf work.

2.3.2 Georges Bank Monitoring Program

The next major environmental effects study of an offshore oil installation in US waters was the on-going Georges Bank Monitoring Program. The active field phase of the Program began in 1981, but was largely cut short by the withdrawal of all exploration platforms from the Bank in August 1982 (Danenburger, 1987). There has been no drilling there since, because of a combination of public reaction, low world oil prices and the fact that the eight wells drilled in 1981-2 were all "dry". In consequence, the Georges Bank Monitoring Program has effectively come to a halt.

This is particularly unfortunate, from the perspective of EEM methodological development, because this Program (and the Environmental Studies Program for Georges Bank of which it forms a part: Leschine and Lahey, 1987) was the first attempt at an on-going environmental monitoring and management process for the offshore oil industry. The Program explicitly combined a long-term regional study with short term, site-specific work around a selected exploration well (Carney, 1987; Leschine and Lahey, 1987). Had drilling continued, other wells might have been monitored, depending on the results obtained at the first one (Leschine and Lahey, 1987). At present, it seems unlikely that there will be such platforms on Georges Bank in the foreseeable future. Certainly, this type of broad-scale planning of EEM promises to incorporate regional questions and the needs of long-term monitoring of oil production from the inception of the exploration phase. It also gives the maximum lead time for the EEM program, which can only aid monitoring efficiency.

There are two problems with the objectives of the Georges Banks Monitoring Program. Carney (1987) has emphasized the lack of a clear definition of the "impacts" sought and has stressed the danger of such a deficiency. The only effects studied, and thus the only ones that can be detected, are in the sediment/benthos system. There are, apparently, plans for adding water column studies in the future (Leschine and Lahey, 1987). In view of the cessation of drilling in 1982, it is unfortunate that these studies were not incorporated into the earlier design.

Despite its several improvements, the program continues other flaws from earlier work, including the concentration on the sediment/benthos system already mentioned and the site-specific station array which differed little from that used at Mid-Atlantic Block 684. The program is essentially subject to all of the criticisms in Section 2.3.1. Many of the other comments made about the Offshore Ecology Investigations, Central Gulf Platform Study and Buccaneer Gas and Oil Field Study (Section 2.1) could also be applied to the Georges Bank Monitoring Program, albeit the errors are less glaring than in the earlier work. In short, the Georges Bank work was a great improvement over its precursors, but it still had flaws.

2.3.3 California Monitoring Program

A proposal to drill production wells off the central Californian coast led to the final offshore EEM program yet to be put into operation: the California Monitoring Program (CAMP: Brewer et al., 1987). This work is a derivative of the Georges Bank program in its conceptual framework and design, though it did not commence until after the exploration drilling had been completed in its area. CAMP has been planned as three phases, with an intended duration of ten years. Phase I (1984-6) was designed as the preliminary to EEM, with the general objectives of describing background conditions and providing the necessary information for quantitative experimental design. Phase II (1986-91) should span the initial production drilling and lead to Phase III once EEM has gathered sufficient information to permit improvements in the design. Drilling at Platform Hidalgo was scheduled to commence in 1987, with Platform Julius to follow in 1989 and

others thereafter (Brewer et al., 1987). At present, further production drilling off California appears to have ceased.

CAMP has not yet proceeded to the analytical phase. There is, however, a declared intention to follow Green's (1979) so-called "optimal design" (Brewer et al., 1987). It is not at all clear how this could be done with the CAMP station array, since Green (1979) assumed only one control and one impacted station. This uncertainty is, however, immaterial since the inadequacies of Green's (1979) design are already well known (Skalski and McKenzie, 1982; Hurlbert, 1984; Stewart-Oaten et al., 1986).

2.4 Canadian Plans for EEM of Offshore Oil Production

As there were until very recently no firm plans for the immediate development of an offshore oil field in Canadian waters, so there are no definite plans for EEM of such a development. There have, however, been a few detailed proposals of which three are reviewed here: (1) a proposed strategy for EEM on east coast fields (LGL Ltd. et al., 1985), (2) a draft monitoring plan for Hibernia, prepared for Mobil Oil Canada Ltd. (Shawmont Martec Ltd. and E.G. & G. Environmental Consultants, 1983) and (3) the extensive Beaufort Environmental Monitoring Project (BEMP: Indian and Northern Affairs Canada, 1985; Indian and Northern Affairs Canada and Environment Canada, 1985; Everitt et al., 1985). These three encompass the current state of Canadian offshore EEM planning.

2.4.1 EEM Strategy for Canada's East Coast

LGL Ltd. et al. (1985) reviewed past EEM programs, much as the present chapter does but without the emphasis on quantitative aspects. Those authors went on to propose a strategy for offshore EEM in eastern Canadian waters south of 60°N, with specific monitoring programs for the Venture gas field and the Hibernia oil field.

The proposed offshore EEM strategy bears little relationship to previous experience, whether in the North Sea, Gulf of Mexico or elsewhere. This is appropriate, since EEM as presently required in Canada is so novel. LGL Ltd. et al. (1985) made three major suggestions:

- 1) the need for careful planning, both a priori and ongoing, together with careful integration of all parts of the program;
- 2) the need for detailed quality control procedures throughout the program;
- 3) the need for pilot studies, particularly to permit efficient statistical design.

LGL Ltd. et al. (1985) proposed EEM programs for oil production at Hibernia and for gas production at the Venture field. In their Hibernia program proposal, they restricted EEM so as to include only three monitoring projects:

- 1) to detect tainting in exploited marine species,
- 2) to detect elevated hydrocarbon and heavy metal levels in sediments, and
- 3) to detect changes in sediment particle sizes and the benthic community structure.

For the first of these they recommended only "opportunistic" sampling (p. 62) which can never be more than semi-quantitative. They did suggest that taste panel studies, designed to detect tainting, be supplemented by assays of Mixed Function Oxidase (MFO: Payne, 1976, 1985) to determine whether the fish have been exposed to hydrocarbons.

The second and third monitoring projects approximated an application of the Sediment Quality Triad approach (Long and Chapman, 1985; Chapman, 1986). That application, however, was compromised by the lack of any prior definition of the concentrations of hydrocarbons or heavy metals that would be judged to represent "contamination" (Carney, 1987). More seriously, LGL Ltd. et al. (1985) did not suggest what measures of "community structure" should be employed, nor the degree of change in those measures that should be regarded as an "effect".

LGL Ltd. et al.'s (1985) proposal limits EEM to only two ultimate effects: tainting of fish consumed by humans and changes in benthic community structure. No water column monitoring may be an acceptable policy decision. There is, however, no scientific reason to suppose that benthic changes are the only environmental effects of offshore oil production.

2.4.2 A Proposed EEM Program for Hibernia

Shawmont Martec Ltd. and E.G. & G. Environmental Consultants (1983) (SMEEG) prepared a surveillance and monitoring plan for Hibernia on behalf of Mobil Oil Canada Ltd. This plan was not developed beyond the draft stage but as it incorporated the only specific Hibernia EEM program proposal, it merits attention here.

The proposed station array was essentially a derivative of that used in the Georges Bank Monitoring Program (Section 2.3.2). This station array is subject to many of the same criticisms as are the arrays employed in the Mid-Atlantic Block 684 Study, the Georges Bank Monitoring Program and the California Monitoring Program (Section 2.3). The fewer stations suggested for Hibernia and their southward bias would have been additional disadvantages.

SMEEG placed the same stress on annual surveys and precise navigation as did LGL Ltd. et al. (1985) and their plan is, therefore, subject to some of the same criticisms. The former's EEM program was, however, more wide ranging and more detailed than the latter's, and included: "Scope for Growth" measurements, the presence of contaminants in the macrobenthos, Gray and Misra's (1979) log normal index of environmental stress and MFO induction in ichthyoplankton. SMEEG's proposed program was therefore more

capable of detecting effects than was LGL Ltd. et al.'s (1985) later design.

SMEGG's plan included one improvement over most of those reviewed in this section; they attempted to estimate the sample size required to obtain a given probability of detecting a change. They used an expression from Green (1979),

$$\bar{x} \Rightarrow \bar{x} \pm t_{1-(1/2)\alpha} \frac{s}{\sqrt{n}}$$

that calculates the sample size, n, needed to achieve confidence limits of a chosen magnitude (for example, 25% of the mean) with a chosen probability (1 - α). It would have been more appropriate to calculate the sample size required for ANOVA to detect a chosen difference (for example, 25% of the mean) between impacted and control sites when α is set at 0.05 and β at perhaps 0.1 (methods for these calculations are explained in standard statistical texts). Nevertheless, the explicit attempt to optimize sample size was a valuable advance in program planning. The required numbers of samples, approximately 20 to 200 for 95% confidence limits of 25% of the mean, are an indication of the scale of field effort required for even such coarse environmental measurements offshore.

2.4.3 Beaufort Environmental Monitoring Program

The final major Canadian offshore EEM planning exercise to date has been the Beaufort Environmental Monitoring Program (BEMP). BEMP is fundamentally different from all of the other programs reviewed here, and it has been strongly influenced by Beanlands and Duinker's (1983) ideas. Indeed, it has yet to become a monitoring program. Like CAMP, BEMP was initiated to monitor oil production operations, after many years of exploration drilling (Everitt et al., 1985). Oil production has yet to proceed in the Beaufort. Thus, although BEMP has been active since 1983, it has not yet had anything to monitor. Two principal reports on this work are by Indian and Northern Affairs Canada (1985) and Indian and Northern Affairs Canada and Environment Canada (1985).

BEMP's approach to monitoring restricts the scope of scientific research. More importantly, they restrict "monitoring" to what is termed "experimental EEM" in the present report. In any case, two features of BEMP might usefully be considered when planning EEM for offshore oil development:

- 1) when selecting what to monitor rather than where or how often some structured, interdisciplinary discussions are essential, and
- 2) as seen in the North Sea (Section 2.2), considerable amounts of research have been required in the Beaufort area before fully efficient monitoring can be undertaken.

2.5 Some Lessons Learned About Quantitative Design in Offshore EEM

After some 20 years of offshore environmental effects studies, the body of accumulated experience illustrates many pitfalls to be avoided in the future. There are several specific lessons however, that serve as positive guidance for the future:

- 1) careful quantitative program design is essential (LGL Ltd. et al., 1985; Section 2.4.1; Carney, 1987),
- 2) that design step must build on the results of pilot studies (LGL Ltd. et al., 1985; Section 2.4.1),
- 3) a major body of knowledge is required, based on scientific research, before monitoring can be fully effective (North Sea work and BEMP, Sections 2.2 and 2.4.3)
- 4) monitoring designs should be based on regional plans (Georges Bank Monitoring Program and CAMP, Sections 2.3.2 and 2.3.3),
- 5) depositional areas should be monitored, in addition to the near-field and general region (Georges Bank Monitoring Program and CAMP, Sections 2.3.1 and 2.3.3)
- 6) designs can incorporate replicate production platforms and multiple control sites (Central Gulf Platform Study, Section 2.1.2), though the mere existence of replicates does not constitute valid replication,
- 7) station arrays based on crossed transects are potentially useful, especially when developed to the level of the Georges Bank Monitoring Program arrays (Section 2.3.2) but Hartley's (1982) suggestion of grid arrays may be preferable,
- 8) stratified random designs could increase efficiency (Gray, 1980; Gray et al., 1980; Section 2.2),
- 9) thresholds for management action, agreed a priori by all parties, would greatly ease the monitoring task (LGL Ltd. et al., 1985, Section 2.4.1).

These points do not lead directly to a new, valid EEM design. This is to be expected, since quantitative offshore EEM is such a novel procedure, at least if it is to follow the definition used herein. Many of the problems and objections raised in this report have only become relevant because the definition and objectives of EEM have changed, particularly since Beanlands and Duinker (1983) suggested that EEM could be used in an experimental context. It is the demand that EEM establish causality in "scientifically rigorous" and "statistically sound" ways (Conover, 1985) that would render EEM impossible. - Sound statistics used rigorously cannot establish causality from EEM data. The solution is to change the definition and expectations of EEM, as much as the methods used.

3.0 POSSIBILITIES AND RECOMMENDATIONS

3.1 Environmental Management Possibilities

3.1.1 Recognition of Problem

The first step to solving the problems confronting offshore EEM is undoubtedly their recognition. As is stressed throughout this report, EEM of offshore oil production is relatively novel. A solid conceptual base cognizant of all potential pitfalls must be established for long-term cost-effectiveness for environmental protection during offshore oil and gas development in Canada.

3.1.2 Inapplicability of Experimental EEM

The type of experimental EEM advocated by Beanlands and Duinker (1983), which forms such a key part of present monitoring objectives (Conover, 1985), is not possible offshore. The necessary statistical inferences cannot be validly drawn in the absence of treatment replication. Beanlands and Duinker's (1983) interpretation of the Popperian "Scientific Method" is not suitable to the high variances typical of environmental data. Thus, present monitoring policies should change. If that change is made appropriately, the task facing EEM program designers could be greatly eased. The place of hypothesis-testing, experimental EEM in current Canadian environmental management practice has been outlined in Section 1.3. The extreme view of Beanlands and Duinker (1983), that environmental impact assessment should be structured as a strict hypothesis-falsifying experiment, has also been described.

The Beanlands and Duinker (1983) approach appears attractive. The scientific understanding of the behaviour of perturbed ecosystems would be greatly advanced by properly designed holistic experiments, if such could be conducted. Unfortunately, the disadvantages of the approach, from an environmental manager's perspective, are equally obvious.² Beanlands and Duinker (1983) would only accept an impact prediction in an EIS if that prediction were structured as a testable hypothesis. Yet, the formulation of testable hypotheses is a difficult process. Furthermore, many socially important, highly probable and intuitively predictable, impacts cannot be tested, for practical reasons such as changes in mean year-class strength of a fishery resource, which can only be measured once per year. While experimental tests of hypothetical primary impacts such as increases in ambient levels of discharged chemicals might appear possible, tests of most impacts that would impinge directly on humans clearly do not. Beanlands and Duinker (1983), amidst many sensible suggestions on other topics, may

² See comments on the relationship between research and environmental management by Hecky et al. (1984), Munro et al. (1986) and others.

have therefore, erected a conceptual framework for EEM that is for the most part non-operable.

In short, holistic experiments and experimental EEM would be a very valuable supplement to environmental management, if they could be performed. They very rarely can be. It is possible, however, to conduct small-scale field experiments with replicated treatments to study the effects of oil on several levels of the biological hierarchy simultaneously (Lane et al., 1987a,b; Crowell and Lane, 1988). Such studies can be supplements to environmental management.

Current Canadian monitoring policy and the emphasis on scientific and statistical rigor should change at the same time. Clearly, statistical techniques should only be used correctly; employed in other ways, they can be misleading. Rigorous limits to their use are, however, likely to be narrower than correct ones, since a large body of statistical research has shown that these methods can yield valid results when some of their assumptions are violated. Similarly, knowledge gained through application of the scientific method should be employed during monitoring activities. Clearly, this application should be made carefully and correctly with a full understanding of the limitations of the knowledge and the methods.

3.1.3 Delimitation of EEM

It is one of the truisms of EEM that no program can measure every environmental variable. Many arguments have been advanced for restricting monitoring to certain types of variables, measuring the effects of only selected anthropogenic inputs.

Scientific information should, of course, always be used, when available, to delimit the range of variables monitored and, hence, to improve the efficiency and lower the cost of the EEM program. It is clear, however, that current knowledge is insufficient for much scientific restriction of monitoring offshore; neither the effects of oil production nor which environments can be safely left unmonitored are known. It might be more appropriate to delimit the range of variables monitored by an explicitly regulated withdrawal of EEM from certain environments and types of variables.

When scientific knowledge does not serve to limit sufficiently the range of monitored environmental variables or anthropogenic inputs, that limitation should be openly based on policy decisions. It should not be founded on arguments that no effect is possible in certain environments. Such decisions might be made on many rational bases but the most obvious is program cost. If an environment or a variable is expensive to monitor and if there is reason to conclude that it is unlikely to be affected or that any effect is unlikely to impinge on human interests, EEM could be withdrawn from that environment. More specifically, if a particular variable cannot be monitored in such a way that a socially important change is detectable, while constrained within an acceptable program budget, then EEM should be withdrawn from that variable as an explicit budgetary

decision (Gray et al., 1980). If this approach was followed, EEM programs would be reduced to acceptable sizes.

3.2 Statistical Possibilities

The solutions to the quantitative problems of EEM are far from being exclusively statistical, but statistics constitute a key quantitative component of any EEM program. Improved statistical approaches can, therefore, be expected to lead to more efficient EEM.

3.2.1 The Need for Inferential Statistics

If the information is to be useful to the decision makers it must be interpretable.

The solution is to base the advice on the relatively-objective, inference-testing statistics, such as ANOVA. Hurlbert (1984) rejected this solution and argued for the use of subjective interpretations of graphs and tables, such as Tukey's (1977) Exploratory Data Analysis (EDA), in preference to inferential statistics. Hurlbert (1984) concluded that some successful ecological research, such as the Hubbard Brook deforestation experiment and much of the work in the Canadian Experimental Lakes Area, had been carried out without conventional statistical inferences. Green (1984) agreed with him. In contrast Stewart-Oaten et al. (1986), Carney (1987), and the present authors argue that for EEM confirmatory, decision-oriented inference-testing methods are essential.

EDA does, however, have an important role in the preliminary analysis of any data set (Tukey, 1977; Stewart-Oaten et al., 1986). Furthermore, with the highly-variable data expected from EEM, more advanced analyses, such as ecosystem simulation models should be based upon a good understanding of the specific ecosystem. It can be useful to analyze the data subjectively before undertaking such complex modelling as an aid to understanding, and then only accepting the results if the subjective and objective approaches are mutually supportive. Such a procedure is not, however, often justified with a simple analysis such as ANOVA.

Hurlbert's (1984) preference for the EDA approach was founded on his conclusion that valid ANOVA is almost impossible in the field of ecology. Stewart-Oaten et al. (1986) argued that the choice is not between EDA and inappropriately applied ANOVA, but between a valid test of inference and the termination of EEM. If no such valid test is possible, EEM is not very useful for environmental management.

In summary, we follow Stewart-Oaten et al. (1986) and Carney (1987) in suggesting that EEM must culminate in an inference test. This test cannot demonstrate causality, but such a test can demonstrate that something has happened that must have had a cause. The test should be valid for the situation in which it is used.

3.2.2 General Linear Model (GLM)

Although the overall culmination of an EEM program must be in the form of a statistical hypothesis test, it is clear that no commonly available, packaged test is fully adequate. The desired analysis must, at the very least, capture the information in the spatial and temporal arrays of sampling stations. This cannot be done, for the types of arrays used offshore, by any t-test or factorial ANOVA. A "custom-designed" test is required.

The obvious basis for developing such a test is the General Linear Model (GLM). GLM presupposes no more than that the observations result from the (additive or linear) summation of a series of terms, one or more of which is a random error term. ANOVA, ANCOVA and linear regression are all special cases of GLM. Carney (1987) provides a brief, clear and accessible explanation. Disregarding the use of linear regression, the simplest GLM-derivative used in EEM has been Green's (1979) "optimal design" ANOVA. The failings of this have been described by many authors (Skalski and McKenzie, 1982; Hurlbert, 1984; Stewart-Oaten et al., 1986) and need not be repeated here.

The GLM-derivative approach that has seen much use in EEM is the Control-Treatment Pairing (CTP) ANOVA pseudo-design (Eberhardt, 1976; Thomas et al., 1981; Skalski and McKenzie 1982; Millard et al., 1985; Palmer and Mackenzie, 1985; Millard and Lettenmaier, 1986; Stewart-Oaten et al., 1986). In this family of designs, measured values at control stations are subtracted from those at impacted stations, and the ANOVA is performed on these differences. If this subtraction is performed on logarithm-transformed data, as is usually done, it is equivalent to calculating the ratio of the variable as measured at the two stations. These CTP designs have great merit, although their statistical validity remains unproven. They may never be shown to be generally valid, in which case, measurements of temporal correlations in the data will always have to precede a CTP ANOVA.

In all of these discussions, it has been assumed that the model will be designed in some manner to test the difference between an impacted area and a control which, if not pristine, is at least unaffected by the monitored production platform. The Central Gulf Platform Study introduced an alternative idea in which the different effects of two levels of anthropogenic stress would be contrasted. In effect, they recognized that a literally unaffected control station cannot be found within the vicinity of a production platform.

Their "two-dose" model avoids one major problem facing the analysis of offshore EEM data. It cannot, of course, do so without some costs to the inferences that can be drawn from the analysis. The approach can have great value, but it is not universally applicable.

Clearly, none of these GLM-based models is adequate for offshore EEM as they are presently formulated. There is a need for statistical development to derive an appropriate model. There are, however, strong indications

that a model could be developed, perhaps incorporating (transformed) distance from the production platform as one covariate, various ecological predictor variables as others, and some pair-differences as data.

No such model, however, can suggest causality in the absence of valid treatment replication (Stewart-Oaten et al., 1986). The other problems of an absence of valid replication also remain, particularly the influence of correlated errors on probability estimates and the lack of a valid divisor in F ratios. These problems may, however, be surmountable by appropriate designs. Millard et al. (1985), for instance, claim that temporal correlations are insufficient to invalidate their CTP design, at least in some data sets.

Finally, Green (1979, 1984) has suggested that multivariate ANOVA (MANOVA) could be used so that a single analysis could incorporate all of the monitored variables at once. There are several problems with this suggestion. Not all of the variables, for example, will require the same station array, nor the same analytical model. In general, for EEM, a series of univariate ANOVA's is preferable to MANOVA.

3.2.3 Box-Jenkins Models and Intervention Analysis

Box-Jenkins Models are specifically intended for the analysis of time-series data. They differ from GLM in that some of the terms involve internal multiplicative relationships, although the various terms are still additively related to each other. Hipel et al. (1978) proposed that EEM (their "assessment of environmental impacts") should be based around Intervention Analysis, a derivative of Box-Jenkins modelling in which extra terms or "intervention components", are used to describe the effects of the intervention of external factors on the time series. For EEM, of course, the aim is to see whether there is a significant intervention component corresponding in time to the commencement of the monitored development.

Intervention Analysis is not, however, very powerful. Lettenmaier et al. (1978) have made a detailed power study of the technique. From their results, it appears that detection of a change equal to 25% of the standard deviation (itself often equal to the mean with marine data), with $\alpha = 0.05$ and $\beta = 0.30$, requires 10,000 data points over five years. The chance of not detecting such a change, β , is still 30%. Furthermore, it is important for all Box-Jenkins Models that the data points are equally spaced in time. For an offshore oil development such as Hibernia, this means that one measurement must be made every four hours at every station for five years. Clearly, this is impractical when sampling from a surface vessel. It might, however, be practical if the particular variable could be monitored by an automatic, moored probe, with data telemetry to the production platform; this level of sophistication is possible for many chemical and physical variables.

There are, however, still two problems with any time-series analysis approach for EEM. First, the only indication of causality that it can provide is the coincidence in time between the development and a change in the data series. Second, Box-Jenkins Models can only detect cyclic

phenomena in the time series. To avoid confounding the trend with any environmental impact effect, this would require a design with control stations, which goes beyond the realm of Box-Jenkins Modelling.

Intervention Analysis per se is, therefore, of limited use for offshore EEM, being applicable at most to physical and chemical parameters close to the platform. When developing the GLM-derived models suggested above (Stokoe et al., 1989), however, consideration should be given to incorporating some Box-Jenkins terms. Cyclic temporal variation may be better modelled in this way than by inverse trigonometric transformation into a linear covariate.

3.2.4 Non-Parametric Statistics

Thomas et al. (1978) suggested using non-parametric methods for EEM analysis, and more recently it has been suggested that such methods could overcome the major statistical problems facing EEM. This is questionable. Non-parametric methods differ from parametric ones in only one way: their results are not affected by the statistical distribution of the data. In all other respects, the non-parametric techniques are no more than analogs of parametric methods. Thus, the choice between non-parametric and parametric techniques has little relevance to the major problems facing EEM, such as the lack of valid replication.

If EEM produces data with a distribution that does not, and cannot be made to, meet the requirements of the preferred parametric methods, non-parametric ones should be used. There are two disadvantages to being forced into this position however: non-parametric methods are almost invariably much less powerful than their parametric analogs, and no non-parametric technique can approach the subtlety possible with a parametric GLM approach. The lack of power results in an inflated β , with serious consequences for EEM. The lack of subtlety also prevents the use of the "customized" models advocated in Section 3.4.2 and reduces EEM analysis to the non-parametric equivalent of few t -tests and correlations.

It is, therefore, highly preferable to use parametric statistics whenever possible, transforming the data as necessary to permit this. Clearly, the chosen or purpose-designed statistical model must be carefully explored, so that both its robustness to poorly distributed data and its responses to data to which it is not robust are known. Simple ANOVA can be very robust to non-normality (Glass et al., 1972), and if the EEM models prove to be the same, the non-parametric approach may be entirely unnecessary (Green, 1979).

3.2.5 Type II Errors in Monitoring

The dichotomy between the statistical objectives of research and monitoring was recognized in the early EEM literature (Eberhardt, 1976, 1978; Gray et al., 1980) but has been overlooked in practical applications. In consequence, there has been considerable confusion between the proper conclusion of many studies, that "no evidence of an adverse environmental effect was found", and the more readily understood, more comfortable, but

erroneous: "evidence was found that there was no adverse environmental effect". Carney (1987) and Millard (1987a,b) have recently returned to this theme. The former gave a particularly clear exposition of the issue whereas the latter emphasized the seriousness of past failures to recognize the importance of β in EEM.

Since research properly emphasizes Type I errors, considerable attention has been given to appropriate critical values of α (often 0.05). β has, in comparison, been ignored. For EEM, Eberhardt (1978) pointed out that developers will usually desire α to be very low so as to minimize the chance of being falsely blamed for degrading the environment. The regulators, on the other hand, will often wish β to be very low, so that real damage will not pass undetected. Setting $\alpha = \beta = 0.01$ would, however, require impractical sampling frequencies offshore; where both the sample variance and the costs of sampling are high. There have been suggestions that appropriate values of α and β could be set by Decision Theory (Millard, 1987b).

3.2.6 Null Hypothesis in Monitoring

To be useful, verification EEM must test the null hypothesis:

H_0 : At location z , effect x has a magnitude no greater than $(1 + \delta)y$, where δ is a safety factor set by the same management procedures that are used to determine that magnitude y is itself acceptable.

This has one major advantage. Since β is high in most EEM programs, null hypotheses will often be accepted. If those hypotheses have been correctly formed, as above, this false acceptance leads to a conclusion that "no evidence was found that environmental effects are no worse than expected" which is at least less misleading than "there are no environmental effects".

3.2.7 Spatial Smoothing, Geostatistics and Kriging

In the presentation and display of spatial data a good deal of interpolation and smoothing is invited by the relative paucity of sampling points in spatial domains of interest. In the past two decades increasing use has been made of automatic interpolating methods (Davis, 1987), such as spline techniques (Eubank, 1987), linear and polynomial techniques and the like. Since the early 1960's a body of spatial interpolation techniques referred to as geostatistics (Journel and Huijbregts, 1978; Henley, 1981; Ripley, 1981) has gained wide acceptance. Beginning with problems of estimating the extent and quality of ore reserves, South African mining engineers and French mathematicians developed techniques collectively called "kriging" (after D.G. Krige, one of the pioneers of the techniques).

There has been attention paid to robust estimation in kriging (Bardossy, 1988) and the use of qualitative, judgmental, or "soft" information in applying it (Journel, 1986b, 1988). Problems associated with transformations to the normal distribution, in order to satisfy underlying

assumptions, have led to subsets of kriging denoted by indicator variables and disjunctive framework. Comparisons of alternative methods have been studied in specific instances (Puentes and Bras, 1986).

Ripley (1981) has also presented kriging in the context of time-series analysis. We suggest that such approaches, combining spatial and temporal frameworks, would facilitate smoothing, interpolating, and interpreting environmental monitoring data. No absolute position should be taken regarding their philosophical correctness. Rather the arguments should be noted and tested numerically, using Monte Carlo or other error analysis techniques.

3.2.8 Sampling and Station Arrays

The various disadvantages of the commonly-used radial transect station arrays have been discussed (Section 2.3.1), and we have already recommended giving more attention to Hartley's (1932) suggestion of using grid arrays. The statistical implications of the grid approach are unclear, since it has so rarely been used, and they must be explored before the approach is applied. Grids would provide suitable data for kriging smoothing. The various optional features of grid designs must also be investigated, including: whether the grids should have rectangular, circular or elliptical boundaries; whether the stations should be regularly or randomly placed and whether station density should be constant or should increase toward or away from the platform. It is suggested that the implication of these alternatives be examined before the station array for offshore EEM is chosen.

Gray (1980) and Gray *et al.* (1980) have suggested that EEM surveys should be spatially stratified. This idea should be explored in conjunction with the study on grid arrays. Effective stratification will certainly reduce the sample numbers required and, hence, the program cost. It is much less certain whether effective stratification is possible in the offshore environment, given the present lack of knowledge of variation patterns and the need for EEM to simultaneously survey a suite of variables. More theoretical and field research is clearly needed before full advantage can be taken of the economies possible through this approach.

Frequent survey repetition is not suggested. Instead, the desirable variables to be monitored should be selected through an integration of policy concerns, ecological modelling, ecotoxicological considerations and the like. The spatial and temporal variation patterns of the chosen variables should then be documented by pilot studies. Subsequently, appropriate temporal sampling frequencies can be determined for each variable. If the required frequency for a particular variable is such that monitoring cannot reduce β to a useful level within the constraint of a realistic budget, then that variable must be deleted from the monitoring program. This aspect of the design process should be pursued iteratively, with the ultimate aim of obtaining a minimum-cost but still effective EEM program.

3.3 Conceptual Framework and EEM Design Possibilities

3.3.1 Scoping

In the scoping for offshore EEM, the objectives must be explicitly stated and there must be a clear distinction between the roles of science and policy. Much of this report should serve as a guide for framing objectives. At a minimum, the EEM program should be designed to detect overall environmental deterioration. It should also be designed to monitor key variables or ECARs (Ecosystem Components at Risk) as have been defined in the Generic IEE for the Northumberland Strait Crossing Fixed Link for Public Works Canada (P. Lane and Associates/Washburn & Gillis Associates, 1988). A broader definition is used here than that given by Beanlands and Duinker (1983) for Valued Ecosystem Components (VECs).

EEM must have clear objectives. These have been discussed by Rosenberg et al. (1981) and Conover (1985), both of whom recommended experimental EEM, but in other respects made valuable suggestions. From their work, the testing of impact predictions or verification EEM as the prime focus for EEM (Hecky et al., 1984) is recommended. This should provide some estimate of the reliability of a prediction and, hence, of the environmental management decisions built upon it. By testing both the prediction and the decisions, it would provide the feedback process demanded by Rosenberg et al. (1981) and Beanlands and Duinker (1983). These tests, supplemented by attempts to predict environmental damage before it occurs via early warning EEM, will also detect unpredicted environmental effects and so aid ongoing management decisions. For this latter objective, less emphasis should be given to measuring environmental damage and more to detecting it quickly and stopping whatever activity is causing it.

It is clear, however, that every human action has some effect on the environment. It is, therefore, necessary to determine how severe an effect must be before it is considered to be of interest. For planning of EEM sampling design, this determination must be made a priori and presumably on an ecological or, for impacts on human concerns, a socio-economic basis (Dicks, 1982; Segar and Stamman, 1986; Wolfe, 1987). Once the magnitude of degradation that is important has been determined, it is essential that the EEM program has sufficient resolution to detect such changes. If this cannot be achieved within a practical budget, EEM should be withdrawn as a management tool (Phillips and Segar, 1986).

3.3.2 Selection of Variables to Monitor

The greater part of past research effort in offshore EEM methodology has concentrated on what (not where, when or how often) to monitor. This is essentially a non-quantitative question and, hence, outside the scope of the present report. The variables, however, should be selected with an awareness of the food web and biological hierarchy of which they are a part. The FEARO panel (Paradine, 1985) suggested birds, fish, and sessile organisms be monitored at the Hibernia oil development project.

In Section 3.1, it was suggested that present scientific knowledge is generally insufficient to state which ecosystem components will not be affected by a particular development. Since complete monitoring of everything is impractical, non-scientific decisions must be taken to determine which variables will and will not be monitored. There is, of course, some extant scientific knowledge that could guide those decisions. For EEM of offshore oil production, however, Carney's (1987) recent review and this report have questioned the validity of much of that knowledge. Thus, there is an urgent need for a re-evaluation of the knowledge base given these two reviews. Such a re-evaluation is not research, but it must be a properly skeptical, scientific process.

3.3.3 Bounding

This is the delineation of the space and time dimensions of both the development and the environment that will be considered by the EEM program. This again is a decision formed at the intersection of science, management and logistics. Bounding is intimately tied to the operable definitions of environmental change and consequently "effect" for the selected variables that will be used for EEM. The definition of "significant" environmental change is inherent to the concept of bounding. This is discussed in more detail in P. Lane and Associates/Washburn & Gillis Associates (1988). How bounding is done will also determine the later feasibility of application of the concepts of Cumulative Effects Assessment and Management (CEAM) (Lane et al., 1988).

In addition to these general comments on the relationship between environmental change and bounding, there are also specific considerations concerning the natural distribution and abundance (or concentration for abiotic variables) of the variables of interest. Once the preliminary selection of variables is made for offshore EEM, it will be imperative that baseline field studies are conducted prior to oil development activities. In terms of temporal bounding, EEM should be able to test the predicted effects of the termination of a project as much as those of its commencement. The last of these may not require EEM to continue unmodified for 20 years, but some variables may need to be monitored for the whole of that time period.

3.3.4 Adaptive Approach

The concept of the adaptive approach encompasses several subcomponents, but its basic tenet is that EEM should be designed to accommodate a wide variety of human activities, both routine and accidental that may be associated with the development, and likewise a wide variety of natural events, and that EEM should be designed from its inception to be flexible. An adaptive EEM design is defined to satisfy the following four criteria:

- i) both routine and accidental events are planned for;
- ii) the monitoring plan must be robust for a wide range of events, and human activities;

- iii) the monitoring plan must be responsive and flexible but not impromptu or inconsistent in regard to methodologies and results; and
- iv) it is usually necessary to integrate data collection with modelling to achieve an optimal level of cost-effectiveness.

3.3.5 Methodology to Understand Cause and Effect

The recent volume on George's Bank (Backus, 1987) gives some indication of the scope and scale of knowledge about a marine area available to environmental managers. The designers of efficient EEM need this type of information. For example, we are still very far from having a thorough understanding of the Grand Banks ecosystem: critical parts of this ecological knowledge must eventually be expressed in the form of models. A variety of higher level models will, in practice, be necessary for prediction, linkage description, variable selection and understanding feedback and causality. Although there is a wide range of environmental models, they will have to be adapted and/or custom-designed to provide the specific support required for EEM.

3.3.6 Quantitative Pitfalls and Their Solutions

Most of this report has been a discussion of this aspect of EEM conceptual framework. Many decisions about statistical methodologies and EEM design could be more efficiently resolved if there was an appropriate field data set. If EEM is to be designed to function in the presence of large variations, it is very important that the local patterns are known (Carney, 1987). Effective EEM demands a low β , yet lowering β is expensive. The design of an efficient EEM program, however, demands detailed knowledge of local environmental variation patterns. This needs to be addressed by additional field studies.

Similar field descriptive studies are also needed to support stratification schemes and seasonal sampling patterns. Sampling for some chosen variable can only usefully be stratified if its patterns of correlation with other variables are known. These are not yet documented, in any detail, for the variables of interest on the Grand Banks. Similarly, efficient seasonal sampling patterns must be designed with reference to the seasonal cycles in the monitored variables. There is some extant knowledge of seasonal patterns at Hibernia or other east coast offshore projects, but not enough. Millard et al. (1985) have suggested that temporal correlations are unimportant in some monitoring data. If this were to be proven true at Hibernia, the EEM sampling and analytical designs could be much simplified. A baseline data set could also be used for this purpose. Data gathered appropriately for time-series and spatial-temporal correlations could also be used to fit network models of cause and effect.

3.4 Recommendations

Eberhardt (1976), Cowell (1978), Green (1979, 1984), Gray (1980) Thomas et al. (1981), Phillips and Segar (1986), Segar and Stamman (1986), Carney

(1987) Wolfe (1987) and many others authors have strongly urged planners of EEM to design their programs efficiently before data collection commences. This need has been stressed throughout the present report, so neither the case for it nor the past evidence of its lack of application will be repeated here.

We conclude with four recommendations as follows:

Recommendation 1: Develop the conceptual framework for the initial offshore oil development project using the eight aspects given in this report (Section 3.3) as a guide. These aspects need to be examined to a level similar to that done here for quantitative considerations.

Recommendation 2: Evaluate the trade-offs of the various possibilities for EEM design in terms of their ecological effectiveness and costs to monitor. This is not an exercise in precision but rather "informed" decision making, which should culminate in some practical decisions for offshore EEM.

Recommendation 3: Using the results of Recommendations 1 and 2, develop the quantitative (theoretical) aspects of the EEM design including both statistical and mathematical modelling and determine the required field data.

Recommendation 4: Using the results of Recommendation 3, plan the implementation of the field studies that are needed for the quantitative aspects of the EEM design.

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VOLUME I
REFERENCE GUIDE TO CUMULATIVE EFFECTS
ASSESSMENT IN CANADA

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A REFERENCE GUIDE TO CUMULATIVE EFFECTS ASSESSMENT IN CANADA

VOLUME I

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NOTE TO READER

This volume is part of a three volume set as follows:

VOLUME I A REFERENCE GUIDE TO CUMULATIVE EFFECTS ASSESSMENT IN CANADA

By Patricia A. Lane, Ronald R. Wallace, Richard L. Johnson and David Bernard

VOLUME II FEASIBILITY STUDY IN CEARC CUMULATIVE EFFECTS ASSESSMENT (CEA) WETLANDS OF THE BOREAL AGRICULTURAL FRINGE OF THE PRAIRIE PROVINCES

By Ronald R. Wallace and Patricia Lane

VOLUME III OVERVIEW OF AGENCIES AND INSTITUTIONS INTERESTED IN CUMULATIVE EFFECTS ASSESSMENT AND MANAGEMENT

By Nicholas Sonntag, Patricia A. Lane, Ronald R. Wallace, Brian M. Marcotte and Stephen H. Janes

Please note that VOLUMES II and III are addenda to the main report (VOLUME I). VOLUMES II and III are available in xerox form upon special request to the Canadian Environmental Assessment and Research Council (CEARC) in Hull, Quebec.

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PREFACE

The Canadian Environmental Assessment Research Council (CEARC) was established by the Government of Canada in 1984 to advise governments, and others, on possible improvements to environmental impact assessment. Through the leadership of CEARC, and other Canadian environmental assessment agencies, the assessment of cumulative effects (CEA) is recognized as essential to the maintenance and preservation of Canadian natural resources and environmental quality. This recognition implies a need for a broad approach and long-term solutions to cumulative effects in the scientific, social and institutional spheres of influence. This Reference Guide is the fourth in a series of CEA studies supported by CEARC (CEARC and U.S. NRC, 1986; Peterson et al., 1987; and Sonntag et al., 1987).

This first attempt by the Canadian Environmental Assessment Research Council to produce a detailed Reference Guide for Canada involved many individuals across Canada and the United States. The study team was constituted so that the various geographical regions of Canada would be adequately represented. Dr. Patricia A. Lane, P. Lane and Associates Limited (Halifax, Nova Scotia), and Dr. Ronald R. Wallace, Dominion Ecological Consulting Limited (Calgary, Alberta), served as co-project managers and were responsible for much of the writing. They also collected information on the Atlantic and Prairie Provinces, respectively. Dr. Wallace was the main author of Volume II concerning the Wetlands Feasibility Study (Wallace and Lane, 1988). Mr. David Bernard and Mr. Nicholas C. Sonntag of ESSA Limited (Vancouver, British Columbia) collected information on the Territories and British Columbia, and they were also responsible for summarizing the agencies interested in CEA and for compiling the CEA methods [Volume III (Sonntag et al., 1988) and Appendix 8.1 of Volume I, respectively]. Mr. Stephen H. Janes of Janes and Associates Limited (London, Ontario) was responsible for the Ontario region and for providing advice on jurisdictional and institutional considerations. Dr. Brian M. Marcotte and his student, Mr. Vidar Neuhof, of McGill University, conducted interviews and reported on CEA in Quebec.

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Dr. Gordon Beanlands, Executive Secretary of CEARC, provided overall direction for the project as Scientific Authority. His insight into CEA in Canada and his encouragement greatly enhanced the Guide. He also provided excellent logistical support to the study team. Dr. Barry Sadler and Dr. Jon O'Riordan contributed invaluable advice as to the study direction and content. Dr. M. Husain Sadar and Mr. Robert Connelly, FEARO, Ottawa, also provided advice on the Guide and facilitated discussion on CEA with other FEARO-CEARC personnel.

The project greatly benefited from a Science Advisory Board which was established to ensure the proper balance and scientific content for the study. The Science Advisory Board consisted of the following individuals: Dr. Robert Rosen of Dalhousie University; Dr. Richard Levins of Harvard University; Mr. Richard Johnson, U.S. Fish and Wildlife Service; Ms. Sally deBecker of Pacific Gas and Electric Company; Mr. Earle Hickey of Environment Canada, Canadian Parks Service (Halifax, Nova Scotia); Mr. Bruce Turner, Canadian Wildlife Service; and Mr. Gary Stewart of Ducks Unlimited (both of Edmonton, Alberta). Mr. Turner and Mr. Stewart allowed the use of their unpublished data for the Wetlands Feasibility Study [Volume II (Wallace and Lane, 1988)]. The study team gratefully acknowledges the assistance of these individuals in offering suggestions on CEA, supplying reference materials, sharing their experiences in CEA, and reviewing our draft report.

Besides serving on the Science Advisory Board, Mr. Richard Johnson of the U.S. Fish and Wildlife Service (Fort Collins, Colorado) contributed greatly to the writing of the U.S. experience (Volume I, Appendix 7.3). He generously shared his considerable insight into CEA. Several of the ideas described in these sections were influenced considerably by communications with Dr. Carl Armour and Dr. Samuel Williamson of the U.S. Fish and Wildlife Service. Ms. Merle Peterson and Dr. Everett Peterson, Western Ecological Services Ltd. (Victoria, B.C.), advised us on their previous work in CEA and offered valuable suggestions on the draft report. They also provided the use of their office facilities while the team conducted meetings in Victoria.

Dr. Ted Manning of the Lands Directorate, Environment Canada (Ottawa), discussed CEA with us and provided reference material for the Wetlands Feasibility Study. Mr. John Donihee of Yellowknife, NWT, met with us to discuss CEA issues specific to his area and the overall regulatory considerations of CEA. Dr. Elizabeth Stull of the Argonne National Laboratory gave us advice on the theory of CEA and the description of the U.S. experience. Ms. Chantel Abou Debs prepared the French resume de l'exécutif on the final version of Volume I.

Approximately 80 other persons in government, academia and industry across Canada participated in the interviews. Each individual who contributed to these interviews is sincerely thanked for this important contribution to the Guide. These individuals are listed in Volume III (Sonntag et al., 1988).

The study team sincerely acknowledges the assistance and contributions from all the above-noted. While we acknowledge those significant contributions, the authors accept full responsibility for the comments in, and conclusions of, our report.

EXECUTIVE SUMMARY

There is a whole class of environmental problems that is presently not well treated by traditional environmental impact assessment methods and existing jurisdictional and institutional arrangements. Collectively these problems can be termed cumulative effects (CE). They can arise from multiple human activities in a given area or from multiple perturbations to the environment from a single activity. Cumulative effects can be characterized as occurring over spatially-extended areas greater than the size of the local ecosystem. Examples in Canada include long-range transport of atmospheric pollutants, global climate change, large water diversion projects, groundwater contamination from toxic chemicals, and habitat fragmentation. Probably every part of Canada is experiencing cumulative effects in one form or another.

Environmental impact assessment procedures and requirements in Canada are narrowly focussed upon single proponent, single development assessments. These are termed proponent-driven assessments. There is no effective assessment and management approach to regional patterns of environmental deterioration which may result from small incremental actions having no identifiable proponent or where there are so many proponents and human activities occurring that there is no way to assess their combined effects on the environment. These are termed ecosystem-driven assessments. Who assumes responsibility in such instances is a key question. Cumulative effects have ramifications beyond environmental deterioration per se. Broad-scale loss of environmental quality implies severe long-term economic losses and a restructured set of development opportunities, or lack of them, for Canada's future. The World Commission on Environment and Development (1987) has recently reported on the ramifications of cumulative effects on a global scale. Canada, as a country both rich and yet dependent on its natural resources for its continued prosperity, cannot permit cumulative effects to increase in an unchecked and unmanaged fashion if Canada is to have an effective sustainable development policy.

The objectives of the Guide are to provide a basic reference to cumulative effects problems in Canada, to describe the present conceptual thinking on the subject here and in the United States, and to present a methodological framework for conducting CEA. A way to categorize cumulative effects problems is also given.

Because the consideration of cumulative effects was placed in environmental regulations several years ago in the United States, Americans have conducted more CEA and developed more methodologies than Canadians. There are no CE regulations in Canada. In the U.S., a large number of federal and state agencies as well as a variety of proponents have been actively working on CEA methodologies. Many methods have been suggested, but the basic conclusion of the U.S. efforts is that cumulative effects assessment should be considered a process and not a single method. The U.S. and Canadian experiences are summarized in Appendix 7.0. At different steps in the process, a variety of methods can be utilized. These are outlined in the Guide and annotated in Appendix 8.0.

The Guide is designed for the Canadian user. Beginning with a typology based upon proponent- and ecosystem-driven types of cumulative effects, the user is asked, "Do you need a cumulative effects assessment?" A decision tree helps the user to answer this question.

The user must then decide whether to use a top-down or bottom-up type of cumulative effects assessment. The Guide provides a 12 to 13 step process for either application. Essentially, the top-down approach is used for ecosystem-driven and the

bottom-up for proponent-driven cumulative effects problems. Central to the conceptualization is the notion of feedback, and early determination of whether the key feedbacks occur with the human activities, the environmental changes they induce the valued ecosystem components, or some combination of these. Feedbacks are very important in these complex systems. Feedbacks can be the cause of surprise and counterintuitive effects that foil management efforts. Feedbacks can also serve a valuable role in system control but only if we can understand and manage them appropriately.

The Reference Guide concludes with three sets of recommendations on further development of CEA in Canada. These include: (1) methodological considerations, (2) potential jurisdictional and institutional arrangements, and (3) the use of consensus-building techniques.

For the first set of recommendations, the main method suggested is loop analysis which centers on the evaluation of feedback relationships and their role in complex systems. Loop analysis involves qualitative network analysis and can be used by a non-mathematical assessor. The method can combine biophysical, socio-economic, jurisdictional and institutional variables in the same cause and effect network. This facilitates the elucidation of the system behaviour emanating from the interconnection of these disparate variables. In this section, we also recommend that the development of CEA will proceed most effectively if there is an iterative process between the improvement of the conceptual framework and its application in a case study format. Both top-down and bottom-up case studies are needed.

In the second set of recommendations, we suggest that there be some new federal initiatives for CEA in Canada. There needs to be an effective combination of regional planning and environmental assessment capabilities in a new type of environmental management. We suggest that five regional CEA boards be established across Canada and that they operate under federal mandate, to guide CEA efforts in their respective regions with an overall cohesive policy on sustainable development. These boards would handle both proponent-driven and ecosystem-driven types of CEA.

In the third set of recommendations, we discuss many cumulative effects problems centering on the joint use and sharing of environments and their resources. This inherently involves negotiation by all parties concerned to ensure the optimal use and least damage to these spatially extended systems.

The Guide also contains a glossary of acronyms and a large bibliography to direct the reader to more specialized areas of the cumulative effects literature. A feasibility study involving the wetlands of the boreal agricultural fringe of the Prairie Provinces is described in Volume II (Wallace and Lane, 1988). Both the biophysical and associated jurisdictional and institutional problems are discussed. Suggestions are then given as to how the cumulative effects problem could be assessed using a top-down assessment process. In Volume III agencies and organizations interested in CEA in Canada are described (Sonntag et al., 1988).

RESUME DE L'EXÉCUTIF

Il existe à l'heure actuelle toute une classe de problèmes environnementaux qui ne sont pas bien traités par les méthodes traditionnelles d'évaluation d'impact environnemental et par les règlements juridiques et institutionnels actuels. Pris collectivement, ces problèmes peuvent être appelés effets cumulatifs (EC). Ils peuvent découler d'activités humaines multiples dans un secteur donné ou de perturbations multiples de l'environnement causées par une seule activité. On peut caractériser les effets cumulatifs par le fait qu'ils agissent sur des zones étendues dépassant la taille des écosystèmes locaux. Au Canada, on peut citer comme exemples les transports à grande échelle de polluants atmosphériques, les changements climatiques globaux, les projets de détournement des cours d'eau de grande envergure, la contamination des nappes d'eau souterraine par des substances chimiques toxiques et la fragmentation des habitats. Il est probable que partout au Canada les effets cumulatifs sont ressentis sous une forme ou une autre.

Au Canada, les procédures et les conditions requises d'évaluation de l'impact environnemental sont axées étroitement sur les évaluations d'un seul soumissionnaire, à développement unidimensionnel. C'est ce qu'on appelle des "évaluations orientées sur le soumissionnaire". Il n'existe pas de façon efficace d'évaluer et de gérer les patrons régionaux de détérioration de l'environnement pouvant résulter de petites actions additionnelles sans soumissionnaires identifiables ou dont le nombre de soumissionnaires et d'activités humaines est si élevé que l'évaluation de leurs effets combinés sur l'environnement soit rendue impossible. Ce sont les "évaluations orientées sur l'écosystème". La question essentielle est de savoir qui assume la responsabilité dans de telles conditions. Les effets cumulatifs ont des répercussions qui dépassent la détérioration de l'environnement proprement dit. Une perte de qualité environnementale à grande échelle implique, pour le futur du Canada, des pertes économiques sévères à long-terme et une restructuration, ou l'absence, de possibilités de développement. La Commission mondiale sur l'environnement et le développement (1987) a récemment présenté un rapport sur les ramifications des effets cumulatifs à l'échelle de la planète. Si le Canada, un pays à la fois riche mais dépendant de ses ressources naturelles pour le maintien de sa prospérité, veut maintenir une politique de développement soutenu efficace, il ne peut permettre l'augmentation des effets cumulatifs sans surveillance et sans gestion.

Les objectifs du Guide sont de fournir des références de base sur les problèmes d'effets cumulatifs au Canada, de décrire les concepts actuels sur ce sujet adoptés ici et aux États-Unis, et de présenter un cadre méthodologique pour effectuer une EEC (Evaluation des effets cumulatifs). Une méthode pour classer les problèmes d'effets cumulatifs est aussi indiquée.

Etant donné que les États-Unis ont, il y a plusieurs années, tenu compte des effets cumulatifs dans leurs règlements sur l'environnement, les Américains ont réalisé plus de EEC et développé plus de méthodologies que les Canadiens. Il n'existe pas de règlements relatifs aux EC au Canada. Aux É.U., de nombreuses agences gouvernementales, des agences des différents états, ainsi que de nombreux soumissionnaires ont travaillé activement sur les

méthodologies des EEC. De nombreuses méthodes ont été suggérées, mais, la conclusion essentielle des efforts américains est que l'EEC devrait être considérée comme un processus et non comme une simple méthode. Les expériences américaines et canadiennes sont résumées dans l'annexe 7.0. Un choix de méthodes peuvent être utilisées à différentes étapes du procédé. Elles sont résumées dans le Guide et annotées dans l'annexe 8.0.

Le Guide est conçu à l'intention des utilisateurs canadiens. En commençant par une typologie basée sur les types d'effets cumulatifs orientés sur le soumissionnaire ou l'écosystème, l'utilisateur doit répondre à la question suivante: "Avez-vous besoin d'une évaluation des effets cumulatifs?". Un schéma aide l'utilisateur à prendre sa décision.

L'utilisateur doit alors décider s'il va utiliser un type d'évaluation des effets cumulatifs allant de haut en bas ou de bas en haut. Le Guide fournit un procédé à 12 ou 13 étapes pour chaque application. En gros, l'approche allant de haut en bas est utilisée pour les problèmes d'effets cumulatifs orientés sur le soumissionnaire et l'approche allant de bas en haut, pour les problèmes orientés sur l'écosystème. La notion de feed-back et la rapidité à déterminer si les feed-backs principaux sont liés aux activités humaines, aux changements environnementaux qu'ils induisent, aux composantes précieuses de l'écosystème, ou à une combinaison de tous ces éléments, sont essentielles dans la conceptualisation de l'évaluation. Dans ces systèmes complexes, les feed-backs sont très importants - ils peuvent être la cause d'effets de surprise et anti-intuitifs qui contrecarrent les efforts de gestion. Cependant, les feed-backs ne peuvent jouer un rôle précieux dans un système de contrôle que si nous pouvons les comprendre et les administrer de façon appropriée.

Le Guide de référence conclut par trois séries de recommandations sur le développement plus poussé des EEC au Canada comprenant: (1) des considérations méthodologiques, (2) des règlements juridiques et institutionnels éventuels, et (3) l'utilisation de techniques menant à un consensus. Pour la première série de recommandations, la méthode principale suggérée est l'analyse en boucle centrée sur l'évaluation des relations du feed-back et de leur rôle dans des systèmes complexes. L'analyse en boucle implique une analyse qualitative de réseau et peut être utilisée par un assesseur non-mathématicien. La méthode peut combiner, au sein du même réseau de causes et effets, des variables biophysiques, socio-économiques, juridiques et institutionnelles. Cela facilite la clarification du comportement du système provenant de l'interconnection de ces variables disparates. Dans cette section, nous faisons aussi remarquer que la façon la plus efficace de réaliser le développement d'une EEC est un procédé itératif entre l'amélioration du cadre conceptuel et son application au format d'une étude de cas. Des études de cas, effectuées en allant de bas en haut et de haut en bas, sont toutes les deux nécessaires.

Dans la deuxième série de recommandations, nous suggérons quelques initiatives nouvelles pour les EEC au Canada. Une combinaison efficace entre la planification régionale et les moyens d'effectuer une évaluation environnementale est nécessaire dans un type de gestion de l'environnement quelque peu nouveau. Nous suggérons que cinq comités régionaux d'EEC soient établis à travers le Canada et qu'ils opèrent sous mandat fédéral, afin de uider les efforts d'EEC dans leurs régions respectives, avec une politique

globale cohésive sur le développement soutenu. Ces comités seraient chargés des deux types d'EEC, orientés sur le soumissionnaire et sur l'écosystème.

La troisième série de recommandations contient une discussion sur plusieurs problèmes d'effets cumulatifs, axée sur l'usage commun et le partage de l'environnement et des ressources. Ceci implique en soi des négociations avec tous les intéressés pour s'assurer que ces systèmes étendus dans l'espace soient utilisés de façon optimale et avec le minimum de dégâts.

Le Guide contient aussi un lexique des acronymes et une bibliographie importante qui orienteront le lecteur vers des zones plus spécialisées de la littérature sur les effets cumulatifs. Une étude des possibilités réalisée sur les marécages de la bordure agricole boréale de la province des Prairies est décrite dans le volume II. Les problèmes biophysiques ainsi que les problèmes juridiques et institutionnels associés, y sont discutés. Nous donnons des suggestions sur l'évaluation des problèmes d'effets cumulatifs à l'aide du processus d'évaluation allant de haut en bas. Les agences et les organisations intéressées par l'EEC au Canada sont décrites dans le Volume II.

1.0 INTRODUCTION

1.1 Cumulative Effects Assessment (CEA)

The purpose of this section is to define CEA and to provide an overview of why CEA is such a difficult subject area both conceptually and operationally. In addition, the importance of improving our ability to conduct, implement, and review CEAs is emphasized. Cumulative effects or impacts potentially affect every Canadian now and in the future. CEA is probably the most difficult and inherently complex problem facing environmental managers, regulators, and developers in managing our natural environments and resources. Effective management of cumulative effects in Canada is the key to a successful policy for sustainable development.

Traditional environmental impact assessment (EIA) usually centers on a single development planned by a single proponent in a circumscribed area, such as a local ecosystem, for a specified time period. In recent years, there has been growing dissatisfaction with this assessment process since many problems of environmental deterioration, resource management, and multi-user conflicts are broader in scope. The growing awareness of the serious long-term effects of acid rain, global warming, urbanization, accelerating loss of wetland habitats, large river diversions, large lake eutrophication, and species extinction have captured the attention of national and international decision makers. These are cumulative effects problems. Because EIAs have traditionally examined a single project and effects that can be directly traced to the project, there is no doubt that considerable environmental deterioration proceeds unrecognized and unchecked.

Cumulative effects assessment (CEA) is currently undergoing intense development in Canada and abroad to address the larger spatial and longer temporal scales for ecological, socio-economic, jurisdictional and institutional systems related to environmental deterioration. An increasing number of literature citations also demonstrates that CEA will have profound effects on how we conduct environmental assessments, influencing information requirements, data collection, analytical tools, environmental training and jurisdictional and institutional systems. Assessments will be increasingly of an interdisciplinary nature and will have to cross traditional jurisdictional boundaries. New assessment methodologies and institutional frameworks will be necessary for Canada to be able to conduct CEAs and manage CEs effectively. The significant challenge posed by cumulative environmental effects will test all levels of the scientific and environmental management communities, not just in Canada, but globally.

The problem of developing CEA is multifaceted. First, it is difficult to achieve consensus on the definition of cumulative effects (CE). Second, many small individual actions are not considered important in traditional EIA even though we now realize that their collective consequences can be severe on a regional scale. Third, the theory of cause and effect in spatially- and temporally-extended ecosystems is in its infancy, and what theory has been developed at the regional level in ecology, such as island biogeography, was not designed to answer EIA-type questions. Fourth, the existing jurisdictional frameworks are not equipped to deal with cumulative effects

problems effectively. These frameworks have existed since Canada became a country and clearly predate the existence and identification of many cumulative effects problems. In addition, overworked environmental managers are limited by their disciplinary training, mandates, geographical jurisdictions and time constraints.

At present, there is no consensus on the definition of cumulative impacts. A working definition can be developed from a description of some possible types of cumulative impacts as follows (adopted from CEARC and U.S. NRC, 1986):

- 1) Cumulative impacts happen over a period of time when the same type of perturbation occurs with high frequency so that the separate perturbations are not damped out by the ecosystem (time-crowding). An example is a continuing incremental input such as acid deposition. This is essentially an example of a periodic input phenomenon.
- 2) Cumulative impacts happen in space when the same perturbation occurs in locations so close together that effects overlap spatially (space crowding). An example is the cyclical effects of forest clearcutting. This is a type of synergistic phenomenon.
- 3) Cumulative impacts occur from different types of perturbations (possibly from separate developments, activities, etc.) that affect similar environmental components if the spatial-temporal scales of the perturbations overlap sufficiently (compounding effects). This type of cumulative impact involves both periodic and synergistic inputs, and we define it as "combined".

It is now recognized that many diverse and remote factors may cause cumulative effects. For example, certain economic sectors may be directed, supported or controlled by governments, regulatory requirements, or jurisdictional factors in ways which synergistically cause long-term environmental degradation. The agricultural industry is a good case. Agricultural policies have provided subsidies, at virtually any cost, for an uncontrolled expansion of the agricultural base. These policies have caused additional and significant fragmentation of wetland habitats, serious soil erosion, and deterioration of aquatic systems through chemical contamination in runoff and siltation.

Cumulative effects are important because of their inherent multiplicative and nonlinear nature; combined effects represent a general example of this phenomenon. Many ecological relationships are essentially nonlinear, and these nonlinearities are magnified under many cumulative effects scenarios. Clearly, if cumulative effects are not addressed appropriately, there can be large prediction errors. Factor interaction is well known in toxicological studies at the individual level, but it is less well understood at the population and ecosystem levels.

Small Local Effects - Big Regional Problems

In 1982, Odum compared the problem of cumulative effects with the "tyranny of small decisions" as described by an economist, Alfred Kahn (National Research Council Committee, 1986). This concept calls attention to the fact that

whenever numerous, small decisions affecting the environment are made independently, the incremental consequences of the decisions are usually not addressed nor are they recognized as being caused by discrete events. As a result, long-term or large-scale environmental perturbations have not been examined by traditional approaches to EIA nor are these problems, which are growing in significance, solvable by these methods. For instance, there are no scientific methods presently available which adequately assess cumulative effects quantitatively.

Theory of Cause and Effects in Spatially Extended Ecosystems

Whereas there is no consensus on the definition of cumulative effects, there is a general realization of the importance of recognizing them and being prepared to assess their significance in real world situations. As noted above, the pervasive nature of acid deposition on large biogeographical scales has called attention to the importance of this class of problem. As Canada becomes increasingly developed, areas larger than the size of the local ecosystem will continue to be affected both periodically and synergistically. As we develop improved conceptualizations of these spatially-extended ecosystems to predict and manage impacts, we will also need to develop new analytical tools to understand causal relationships correctly. Conceptualization will be an iterative process as analytical capability also improves. The environmental assessment community is now at a stage of discussing the issues and understanding the problems, but there is no universally established approach presently available and readily usable. If a generalized approach could be developed, then the whole field could progress quickly. Intensive theoretical development is necessary to expand the analysis of local ecosystems and singular developments into larger space and time scales for multiple developments and more intricate levels of causality that simultaneously consider the biophysical and socio-economic aspects of the environment simultaneously.

In addition to the increased complexity of larger time and space scales and more intricate causality, there is also a more complex level of prediction required for CEA than for traditional impact assessment. In the latter, we wish to predict and compare system structure and behaviour with and without a particular development. Often the development can be described using engineering design criteria. With CEA, we will need to predict not only the future behaviour of systems with larger space and time scales but we also will be required to predict cumulative effects from developments not yet proposed but probable for a given region. This is necessary so that environmental planning is optimized over the long term and resources are not used on a first come, first served basis, which would close options for future development and resource use.

Resolving cumulative effects is not simply a problem of developing better analytical tools and improved understanding of spatially-extended and perturbed ecosystems. Equally important is the plethora of jurisdictional and institutional barriers that presently impede the identification and management of cumulative effects. Hence, resolution of CEA in Canada will be significantly influenced by the solutions chosen for scientific, social and jurisdictional problems. While partial solutions in individual categories can

be achieved, successful resolution of CEA problems is unlikely unless problems are addressed from an integrated perspective. Identifying workable solutions to the jurisdictional, institutional and procedural problems which impede implementation of environmental problem solving has only just begun to be addressed rigorously in Canada.

There is probably no single solution to all the types of environmental problems which may arise. The point, however, is that a solution to the existing jurisdictional dilemmas is presently being sought by several agencies. It is also clear that, while methodological problems for CEA may be identified and scientific approaches to resolve them instituted, the jurisdictional barriers may prevent, or at least frustrate, their optimal implementation. For example, we presently understand a great deal about the underlying causality and scientific aspects of impacts related to acidic deposition, but jurisdictional and institutional considerations have hindered the implementation of needed solutions.

As Peterson et al. (1987) pointed out, the structures of the existing environmental management agencies in Canada may be not capable of responding to, or identifying, cumulative environmental effects:

"Today's dominant social and economic perspective is that if cumulative effects are either not identified or are ignored at the biophysical level, the next point at which identification will occur is when cumulative social and/or economic effects on the human population are identified. For example, scientific evidence suggests that proper containment and disposal of toxic wastes is imperative. However, the institutional structures to accomplish this commonly fail until an example of effects on human health results in public awareness."

This example of the multiplicity of jurisdictions and institutions failing to catch environmental problems of significance until they reach public awareness by threatening health, is compelling.

It is recognized that these types of agencies cannot be expected to respond to broader issues which extend through and beyond their collective jurisdictions. Indeed, in Canada the existing institutional arrangements tend toward a fragmentation of interests and responsibilities regarding renewable resource management. No better example can be cited than the traditional struggles between forestry and fisheries interests and agencies. Harvested forest lands erode into, and degrade, fisheries habitat. Forest conservation and protection measures by aerial pesticide spraying against forest pests may directly affect aquatic species and nearby communities. Yet, no area wide management agencies are able to resolve, or perhaps even address, the wider issues of long-term management of the collective resources. Often, institutional arrangements preclude the open and objective resolution of such conflicts and leave little access for the average citizen, except through the costly and largely inefficient mechanism of the courts.

Similarly, societal attitudes toward the re-use (or non-use) of materials greatly amplifies the long-term, cumulative effects of seemingly

insignificant, individual choices. Hence, economics, society and human attitudes all act, and interact, to produce cumulative effects on the receiving environment. This implies that effective solutions must address our fundamental attitudes toward the conservation and management of natural resources and environmental quality, and must be framed in policies that are cognizant of the interrelationships of environment, economics and society, that is, sustainable development. In short, CEA may be defined by environmental management techniques but cumulative effects can only be fundamentally and effectively addressed by much wider economic and social policy decisions. It is also essential that the management framework be designed for the extended spatial-temporal scales that are inherent in cumulative effects.

In any case, there has been no successful demonstration of legislative or institutional arrangements in Canada which has been able to cope adequately with, or regulate, the major long-term cumulative trends in environmental impacts at a national or regional scale. New legislative and institutional arrangements are urgently required as are the overall CEA conceptualization and approach that must underlie them.

1.2 Objectives of the Reference Guide

The objectives of the Reference Guide are as follows:

- 1) To motivate potential users to consider CEA-type problems and their solutions important for ensuring the quality of the Canadian environment,
- 2) To provide a basic reference of CEA terminology, concepts and references,
- 3) To present a holistic approach including a methodological framework for CEA and for organizing associated information,
- 4) To present an overview of major cumulative effects in the five geographical regions of Canada,
- 5) To give a brief review of the history and emerging theoretical framework for CEA in Canada and the United States using case studies,
- 6) To provide recommendations for future development of CEA in Canada,
- 7) To present a CEA feasibility study centering on wetland habitat fragmentation of the boreal agricultural fringe of the Canadian prairies (Volume II - Wallace and Lane, 1988), and
- 8) To summarize future collaborative potential for CEARC with other agencies and organizations interested in CEA in Canada (Volume III-Sonntag et al., 1988).

1.3 Application of the Reference Guide

This Guide centers on the environmental aspects (mainly biophysical) of cumulative effects assessment. It was outside the Terms of Reference of the study to provide a complete methodological framework for the related socio-economic, institutional and jurisdictional areas. We do, however, point out many of the concerns in these latter areas throughout the text and illustrate how they relate to the biophysical considerations.

Human activities and developments are pervasive throughout much of the Canadian environment. Some types of impacts, such as those related to acidic deposition, may originate thousands of kilometres away from the affected area. In a strict sense, probably every square metre of the Canadian environment is cumulatively impacted. A single development, such as a power plant, can impact, in cumulative ways, on a local ecosystem through simultaneous chemical releases, thermal plumes, impingement and entrainment processes. Thus, a particular environmental component, like a larval stage of a valuable fish species, can experience multiple and cumulative effects via a single development in a local ecosystem as well as from multiple developments or other human activities located both inside and outside its watershed.

For the above reasons, it is necessary to focus this Reference Guide on the class of CEA problems beyond the level of a single development in a local ecosystem (which can be handled by traditional EIA procedures) and up to and including all of Canada. Thus, spatially-extended systems above the local ecosystem form the smallest unit, and all of Canada is the largest unit considered by the Guide. For example, we consider the role of worldwide climatic change within Canadian borders but the Guide does not extend beyond Canadian territorial boundaries to consider cumulative effects of climate change on the biosphere. Often, CEA problems will involve two or more jurisdictions although presence of multiple jurisdictions is not a prerequisite for CEA. We only briefly consider international jurisdictional overlaps, such as the management of the Great Lakes by the International Joint Commission, when such examples enhance description of potential methodologies and conceptual advances.

There is a wide variety of potential users of this Guide across Canada. In developing the Guide, we consulted personnel from several tiers of territorial, provincial and federal regulatory agencies, industry, academia, consulting companies, municipal government, and even several U.S. scientific and regulatory agencies. All expressed interest in obtaining and using the Guide. Obviously, it is difficult to provide a single guide that will simultaneously serve the field ecologist and the senior policymaker. They work on different aspects of CEA problems and have different types of training and perspectives. Each type of knowledge is important in the total approach to CEA, but the key point is that the knowledge and the needs for additional insight are different.

No one guide can be everything to all users, but, to slant the Guide to one type of user would necessitate neglecting many others. Therefore, we have chosen a compromise approach, but we have had to assume that readers have a basic understanding of how environmental impact assessments are conducted in

Canada according to federal and provincial procedures. For example, we have assumed the reader is familiar with, and has access to, FEARO assessment guides and Beanlands and Duinker's (1983) "An Ecological Framework for Environmental Impact Assessment in Canada". We have also assumed that readers will be familiar with such basic assessment methodologies as ad hoc, checklist, mapping and matrix approaches. Although we discuss several potential modeling approaches for CEA, we assume that readers do not have extensive mathematical and modeling experience or experience with multi-jurisdictional assessments for which no clear guidelines may exist.

As in any new developing field, the terminology of cumulative effects assessment is evolving rapidly. The literature contains a large number of terms and acronyms that are difficult to remember so we have included a glossary (Chapter 5.0) to assist the reader. We have also tried not to encumber the text with too many definitions. Each acronym is explained when it is first used. In Chapter 6.0, extensive literature references are provided so that the reader can follow-up his or her particular interests in CEA.

Eventually, it may be desirable to produce custom-designed reference guides for particular audiences or specific types of CEA problems. While CEA is developing so rapidly, however, it is beneficial to provide all interested parties with a common, generic information base.

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2.0 CUMULATIVE EFFECTS ASSESSMENT -- AN OVERVIEW

2.1 Global Sustainable Development and Cumulative Effects

To date, Canada has not had an organized approach for recognizing, evaluating and managing cumulative effects. Overall, the focus on EIA has been narrow and proponent-driven. Types of proponents have also been limited. Recently, the Minister of the Environment called for review of the environmental assessment process in a Green Paper (PR-157). Although the changes suggested therein are needed, they still do not provide for an operative cumulative effects assessment and management (CEAM) framework. This means that many types of environmental problems will continue to slip through the assessment net. While EIA changes are being suggested is the opportune time to broaden our view of environmental problems and to plan more wisely for the future. The recent report of the World Commission on Environment and Development has provided a compelling rationale for combining long-term economic development and environmental management into an integrated framework or "blueprint for global survival". The Commission, however, did not supply this blueprint. Undoubtedly, the blueprint will be constructed and applied in many contexts throughout the world in the next few decades. If Canada is to manage her environments wisely, as well as be a full participant in promoting and implementing this global blueprint, then effective CEAM must be given priority. The large-scale environmental deterioration the World Commission refers to is, in essence, global cumulative effects. The blueprint is CEAM.

Creating the blueprint will involve conceptual innovation in integrating biophysical, socio-economic, and jurisdictional and institutional considerations on extended spatial-temporal scales into a holistic assessment and management framework. This framework will need to be applied in appropriate case studies that will iteratively lead to enhanced understanding. This will come about in part by combining aspects of impact assessment and ecological principles with aspects of regional planning and economic development in a novel manner. It is too early to envisage whether or not a new discipline will evolve, but it is a possibility. At the very least, a new thinking and a new determination to implement CEAM will have to emerge. They will have to be a form of goal-oriented planning where the goals are no longer the more simplistic, short-term economic ones of traditional regional planning and cost-benefit analysis. The goals must be environmentally based with clearly specified economic ramifications. The overall goal, of course, is long-term environmental sustainability which also includes an economic optimization. Environment and economics are inextricably linked in a fundamental way; all of our needs must be met from our environment. To achieve environmental sustainability, we need a new set of criteria that are conceptually sound, operationally measurable, and implementable in either existing or innovative jurisdictional and institutional arrangements.

This Reference Guide centers on a portion of the necessary blueprint for sustainable development, that is, the largely environmental and ecological components of the conceptual base. In this chapter, we first describe CEAM in Canada and how it relates to EIA and environmental planning. Next we discuss some of the institutional and jurisdictional barriers to effective CEAM in Canada. It was beyond the Terms of Reference to make detailed proposals for

the elimination of these barriers. In this chapter, we also list the basic assumptions of the conceptual framework. Assumptions such as these should always be made explicit.

2.2 CEAM in Canada

As some of the following chapters will show, cumulative effects are very much a part of a pervasive deterioration to the Canadian environment. CEs affect overall environmental quality as well as many resource management issues. CEs have always been with us, and they will always be with us. As future increases in population and development occur, however, cumulative effects problems will become more apparent; and they will require better planning and management, especially on a regional to federal level.

Interestingly, some of the problems which result from cumulative effects have been defined for some time, although succeeding generations have approached their solution in different ways. The first National Parks in Canada were areas clearly identified then as national resources worthy of long-term protection and conservation. This recognition, originating in the 19th century, of long-term degradative effects from development pressures, surely constitutes one of the clearest visions of a people's determination to offset cumulative effects. And yet, even that vision could not foresee the magnitude of the effects which were to be created by the industrial revolution, effects which transgress national and international boundaries far beyond the control of our present governmental apparatus.

Although CEA has been a part of environmental management and thinking in Canada, the more formalized conceptual development and plans for implementation are relatively new. Thus, attention is now focused on CEs in a way that has never occurred before in Canada.

The recent history of the CEAM concept can be traced, in part, by examining a few of the more recent and important events in this area. In 1982, Odum (1982) wrote an article pointing out that seldom are the combined consequences of many small, independent decisions affecting ecosystems given much attention. The following year, the U.S. National Research Council formed the Committee on Applications of Ecological Theory to Environmental Problems (CAETEP) which promptly initiated a series of case studies, some of which illustrated cumulative effects problems. These case studies were described in the book Ecological Knowledge and Environmental Problem-Solving (Orians et al., 1986). Meanwhile, CEARC was formed in 1984, and the members soon assigned CEA issues a high priority.

Lack of a universally accepted definition has been a problem for those trying to deal with cumulative effects. To help clarify the basic concept of CEA and to investigate related issues, a major workshop was held in Toronto in February 1985, sponsored by both CEARC and CAETEP. At this workshop, thirty participants, primarily researchers and environmental managers from Canada and the United States, described their perspectives on CEA, and proposed directions for both research and planning (CEARC and U.S. NRC, 1986).

Although proceedings of the Toronto workshop represent a binational perspective on CEA, the recommendations were not synthesized into a definite program of action. Thus, following the workshop, CEARC commissioned two studies to provide recommendations that would help in formulating a multi-year plan to guide Canadian research on CEAM. Together, these two studies (Peterson et al., 1987; Sonntag et al., 1987) defined the state of CEAM in Canada. More recently, CEARC synthesized these two reports into a draft research prospectus (CEARC, 1987) that outlines CEARC's multi-year research agenda on CEAM. The first priority on this agenda was to prepare a Reference Guide for individuals charged with preparing an assessment of cumulative environmental effects, which led to this report.

As Oriens (1986) pointed out, to go beyond managing cumulative effects on a case-by-case basis, scientific and institutional approaches will both require restructuring. He added that considerable improvement in communications between scientists and managers will also be required. One step toward this new state of affairs is to develop a conceptual framework within which CEAs can be conducted.

In their reports to CEARC, both Sonntag et al. (1987) and Peterson et al. (1987) developed conceptual frameworks which they then used to evaluate and discuss cumulative effects case studies (several of which are described in Chapter 3.0). Taken together, these two reports offered several recommendations, including: development of research programs focusing on institutional and management aspects of CEA; evaluation of the usefulness of existing methods and analytical tools for CEA; and testing of the CEA concepts, framework, and methods in a feasibility study.

Using information from these two reports, CEARC is developing a conceptual framework for CEA. There are a number of common elements developed by the two study teams including: systems approach, specific choice of space and time bounds, consideration of selected ecosystem components, sensitivity to thresholds, and consensus. Each of these elements is discussed more fully below.

Although it is not a novel suggestion, it is important that any attempts at CEA adopt a systems approach. Without doing so, it is likely that the evaluation will fail to incorporate effects that accumulate over space or time just as early non-systems approaches (for example, EIA for individual projects) failed to recognize factors that we now recognize as contributing to our current environmental problems.

Likewise, selecting appropriate bounds on the spatial and temporal domain in which the analyses are carried out is important; until the bounds are selected, it is far too easy to focus attention on only some parts of the system rather than on the entire system. Equally important, choosing bounds gives investigators a rationale by which to make decisions on what to include and exclude in a given assessment.

A major step in making a CEA manageable is to accept that, although "everything in ecology is connected to everything else", not all connections are equally important (Orians, 1986). Consequently, only selected ecosystem components and processes need to be considered in CEA. Of course, selecting which components and processes are important is not a trivial task, but there are methods to assist in making these choices.

One area that clearly helps distinguish some types of CEAs from EIAs is consideration of ecosystem thresholds in the former. If CEA results are to be meaningful, some attempts will have to be made to determine what affect a given human action will have on an ecosystem, relative to the internal thresholds of that system. Westman (1985) has pointed out the difference between thresholds inherent in a system and those affecting human values. This relatively new way of looking at impacts promises to assist not only in dealing with cumulative effects issues, but also with questions of significance that have long plagued individuals charged with conducting environmental assessments.

Finally, there is now a recognized need to develop a consensus on approach to CEAM in Canada among the various individuals and organizations involved either in controlling actions that cause cumulative effects, or in evaluating and deciding about the acceptability of these actions.

Our present system of environmental impact assessment addresses mostly single-proponent development and then often those developments which are largely funded by federal funds. A large number of activities that are currently ongoing in Canadian environments are not "caught in the assessment net", and they proceed unchecked. Especially troublesome is the problem of small incremental changes. At a fundamental level, the changes are not different from the biophysical and socio-economic ones found in traditional EIA. What is different is the larger spatial (and sometimes temporal) scales that translate into regional patterns of deterioration and a much reduced set of options for the future of any particular area in Canada. We cannot abate all change and all deterioration, but on a regional level we could certainly implement a much better cumulative effects assessment and planning system than we presently do. This should be done as soon as possible, and it is opportune that attention is presently focused on revision of the Federal Environmental Assessment and Review Office (FEARO) procedures and requirements.

At present, environmental planning is needed at both the provincial and federal levels. British Columbia, Alberta, Ontario and Quebec have formalized environmental and regional planning, but even these four provinces have no formalized frameworks to deal with CEs effectively. To date, mostly ad hoc responses to CEs have been the norm, and there have been few examples of CEA used in standard FEARO practices. Lane and Gillis (1988) did incorporate a simplified form of CEA in assessing the interaction of the proposed fixed link between New Brunswick and Prince Edward Island and twelve other types of human activities in the Northumberland Strait area.

In Canada, the legal and regulatory definitions which might be most relevant to the issue of cumulative impact assessments are not to be found within any single legislative base. Recent initiatives by Environment Canada for

national environmental protection policy may, however, if passed by Parliament, begin to address such questions. The Canadian approach to CEA has tended to rely heavily on the co-operative nature of federal and provincial agencies in meeting the demands of elected officials for environmental protection through inter- and intra-agency initiatives. The new habitat protection provisions of the Canada Fisheries Act is a de facto recognition of the limits to enforcement actions under the Act and the need to adopt a broader perspective in co-ordinating essential ecological factors into renewable resource management. Fisheries and Oceans Canada has further pursued this initiative by formulating a policy on Arctic Marine Conservation Strategy and by implementing newer approaches to fish population management and protection on the east and west coasts of Canada.

The Lands Directorate of Environment Canada has also begun independent initiatives for research on broad scale land management practices. Their strategy paper "Toward Sustainable Land Use" (Manning, 1986) recognizes that the concept of sustained land use, "which maintains longer-term productivity and keeps open as many options as possible for future generations", is essential to the fabric of the nation. As Manning (1986) noted: "Land-use decisions are by their very nature long-term decisions".

This tacit recognition of the scope, extent and timing of development decisions entails an implicit understanding of CEAM. Unfortunately, management control is seriously undermined by the growing complexity and diversity in environmental management agencies. Ten provinces, two territories and a federal government, each having separate environmental legislative bases, present a significant challenge for any manager seeking recognition of, and action in, CEAM. There are some positive signs, however, which indicate that approaches to CEAM across this jurisdictional base may be possible. The Canadian Council for Resource and Environment Ministers (CCREM) has recently launched important studies on the nature of various jurisdictions with regard to regulations on environmental parameters. This basis for integration could be expanded into a "CEAM forum", under which consistent definitions and approaches are applied.

FEARO is another vehicle for potential recognition of CEA. A recent review by assessed the ability of FEARO to affect Canadian decision making on long-term development decisions, and concluded that at the federal level, the process had exerted a significant influence. Interest in the Lancaster Sound Planning Process grew from the recommendations of the Panel. This Planning Process is, in effect, an attempt by federal government to deal with long-term influences of development and to manage cumulative effects management in a unique area of Canada. CEAM initiatives and examples, however, cannot disguise the fact that there exists no single base for CEAM, although significant research initiatives have begun across Canada such as the Commission on Great Lakes Water Quality Agreement, the Commission on Atmospheric Pollutants, and Alberta's Acid Deposition Program.

Wallace, R.R. (1986).
Assessing the assessment process on federal decision-making.
Arctic 39(3):240-246.

2.3 Jurisdictional, Institutional and Disciplinary Barriers to CEAM

While environmental concerns are key to establishing a workable framework for CEA, political institutional, jurisdictional and disciplinary factors must be recognized as exerting a formative influence on the development and implementation of CEA methods. Most of this Reference Guide deals with the ecological aspects of CEA. The purpose of this section is to illustrate that as we develop these facets of CEA, we cannot forget the jurisdictional and institutional problems that must be effectively resolved if we are to assess and manage cumulative effects successfully.

Peterson *et al.* (1987) dealt with the organizational and political aspects of the management component in a conceptual framework for CEA. Those authors noted:

"... There are a number of political factors that cannot be ignored in management of potential cumulative effects. The first is that a political will to act is required before any institutional arrangement can be put in place or before any substantiated decision can be taken. A corollary of this is that the cumulative effects management system must be accountable to elected representatives."

They further expanded this theme with appropriate reference to the democratic context within which CEAM must function if it is to be recognized and sustained:

"Decision-making does not take place in a vacuum. For example, political realities related to unemployment may over-ride environmental considerations. In addition, decisions are made in the prevailing political climate. Because political considerations are very influential, group and individual behavior is of vital importance in any attempt to change or add to the everyday management system."

These basic considerations are highly relevant to any discussion about the jurisdictional aspects which affect CEAM, as they underline the fundamental workings of the democratic process of which environmental management is but one component. Hence, the "interconnectedness" of the political-scientific-social system, which ultimately both produces and manages cumulative effects, must be recognized and carefully assessed if remedial processes are to be successfully implemented. At present, the existing institutions for environmental management are clearly oriented to more traditional environmental impacts. These organizational "barriers" to CEA approaches in Canada are, in turn, reflections of the environmental and jurisdictional realities within the nation.

These traditional approaches assume government intervention either through regulation or by indirect, legislative control over resource allocation. Peterson *et al.* (1987) concluded that attempts to control cumulative impacts through legal liability are unlikely to be successful simply because of the enormous difficulty of attributing those effects to all the parties

potentially contributing to them. In short, due process requires proof of causality and usually sets limits to liability in both time and space.

At the other extreme, cumulative effects defy the best efforts to co-ordinate and manage the plethora of government departments necessary to properly address them. Because of the pervasive nature of cumulative effects, almost every part of government is involved in one way or another. For example, the Department of Indian Affairs and Northern Development (DIAND) produced a large chart in 1982 which attempted, by traditional methodologies, to integrate and co-ordinate all of the departments needed for regulation of proposed oil and gas development in the Beaufort Sea (Peterson et al., 1987). The process used by DIAND included 20 different approval pathways, several reaching to Cabinet level, and 89 boxes indicating regulatory activities contributing to approval decisions. In addition to being unbelievably complex and virtually unworkable, such approaches may discourage developers, proponents or the public from taking environmental management seriously. At the very least, organizational co-ordination at this level of complexity may simply defeat any attempt to constructively address CEs.

Proponents are also reluctant to pursue CEA because their knowledge is limited to their own project. They do not believe they should develop assessment capabilities and be responsible for other projects in their area. They seek environmental approval on the independent merits of their own project and do not want to be constrained by impacts and perhaps wrongdoings of other developers.

Existing institutional structures further present barriers to CEA because any new arrangements must, by definition, be accommodated within the existing arrangements. In turn, these must either be included in existing legislative mandates for EIA or have new ones legislated. The latter becomes an increasingly difficult option, as evidenced by new federal requirements for regulatory impact statements to precede new legislative initiatives. Furthermore, many agencies such as the National Energy Board (NEB) have "quasi-environmental and socio-economic roles" which have a far more powerful legislative base through their hearings process than many environmental agencies such as DOE or FEARO. These latter agencies rarely, if ever, impose specific conditions, by law, on developments outside of existing regulatory compliance requirements. In addition to the institutional barriers to effective CEAM, proponents do not generally consider themselves required by law to address cumulative effects issues and, in fact, legitimize that perspective by "reductionist" approaches to the assembly of EIA (long-term, low-level effects are simply dismissed as being of "moderate" or "negligible" effect, as in the Beaufort Sea EIA) (Peterson et al., 1987).

The real dilemma environmental assessment faces is that a relatively mature and sophisticated procedural approach has emerged without the evolution of any clear-cut ability to integrate the management of the conditions or findings of the CEA research. In Ontario, the review process defined by legislation clearly sets the stage for CEAs and the regulatory body, in probing submissions, has extended its reviews and conditions of approval to reflect the CEA submission. A glaring weakness, however, continues to exist in the institutional system's inability to integrate and carry out the conditions or

findings of the CEA. While the system for environmental review and regulation can spell out conditions, once the project is approved there are few cases where ongoing performance audits occur which can feed back adequate information into the regulatory system to carry out the assessment of cumulative effects.

Part of this deficiency lies in the narrow definition of the jurisdiction for these institutions responsible for carrying out EIAs and arranging the conditions required for the approval of individual projects. Part of the deficiency also can be traced to a fundamental characteristic of CEA findings in that one aspect of a proposal or one environmental condition can precipitate another hitherto unknown effect. Usually the triggering event or effect is well-known and its management well-defined. Cumulative or related effects, however, may not be perceived clearly until long after the triggering event, and there may be no provision available for management of the "surprises". In fact, there may be an absolute refusal of the affected institution to take any action.

While jurisdictional barriers often, and properly, attract much attention in discussions of CEAM, the question of inter-disciplinary problems often goes unaddressed. Each discipline of science (in environmental management) approaches problems from a distinct perspective, and it is this philosophical perspective which may shape subsequent conclusions and definitions of impact. (For example, see the comprehensive review of cost-benefit assessments in air and water pollution control questions in Freeman, 1982).

Engineers approach cost-benefit evaluations of water control structures from the perspective of initial capital cost versus the probability of loss of the structure. Frequency analysis of maximum probable events which could damage structures, however, ignore more subtle long-term effects such as habitat destruction or change (such as siltation/sedimentation effects). Hence, the initial definition of "cost" may fundamentally shape the eventual impact of projects so constructed. Moreover, the weight of the numerous factors shape the definition of "risk". For example, losses to habitat which might prove disastrous to wildlife populations might be judged an "acceptable risk" because of some advantage to humans. One need only examine the consequences and costs from the Bennett Dam in British Columbia as it affected the Peace-Athabasca Delta in Alberta to appreciate the magnitude of such losses. To be sure, the environmental costs of such engineering ventures are beginning to be quantified, but generally through legal remedy after the impacts have occurred. For example, refer to the comprehensive analysis of the Churchill-Nelson Diversion and associated impacts on the native fishery of northern Manitoba (Lehman, 1986). In cases such as these, the long-term "costs" of the engineering works to the renewable resource base may exceed the initial capital costs of the construction, when one considers total legal and compensation settlement costs.

2.4 Assumptions Implicit in Developing a CEAM Framework

In formulating a conceptual construct, whether it is an approach to CEA as given here, a formal mathematical model, or a statistical analysis, it is

useful to list the assumptions. Five assumptions inherent in and fundamental to the CEA approach proposed in this Reference Guide are as follows:

1. Cumulative effects assessment is a complex systems problem. Science has traditionally not been very successful in solving complex systems problems. As our success has increased in solving simple problems, the relative proportion of complex system problems needing resolution has increased. General Systems Theory has provided an overview and approach to this type of problem, but not a detailed methodology (Weinberg, 1975). Science, for example, has been good at analyzing problems of small number systems. These systems have few components and few interactions between components. In engineering approaches where exact measures and complete functional descriptions are possible, small number problems can be solved. Population ecology essentially represents an analytical approach to such small number systems. Use of this approach at the ecosystem level, however, has been limited to theoretical or over-simplified models, a result of the near impossibility of obtaining measures of all the interaction rates and parameters needed for a functional, ecosystem-level model.

For large number systems, those that have a large number of components and many interactions, statistical analysis can be employed if randomness can be correctly assumed to apply and the system can be described by averages or mean values. The gas laws present an example where statistical analysis is applicable. Eighteenth century physicists at first tried to explain the position and velocity of each molecule of a container of gas in their enthusiasm to determine the fate of the universe. This quickly became impracticable as 10^{23} molecules of gas could be present in a single vessel and hence mean values were found useful in formulating the gas laws relating temperature, volume and pressure. Although ecosystems can have a large number of parts, there are not enough components to average them in a meaningful way. A fish is not dynamically or functionally equivalent to 10^6 bacteria even though they may weigh the same. Thus, statistical approaches are useful in ecology only in certain restricted applications.

Between small and large number systems there are middle number systems, those that have intermediate numbers of components and intermediate levels of interconnection. These are the systems that have too many parts for pure analytical approaches to work and too few parts for statistical assumptions of average properties to be valid. As the number of components increases arithmetically in a system, the number of interactions increases geometrically. Scientists and social scientists have few reliable tools to deal with this middle level of complexity. For middle number systems, we find that the counterintuitive results of Lane and Levins (1977), the surprises of Holling (1982), and even the folklore wisdom of Murphy's Law apply. Murphy was reputed to have despaired over an inept aircraft technician when he stated, "Anything that can go wrong, will go wrong". CEA is a middle number system problem, and we cannot expect traditional methods to work well for it. Simplification, through the use of appropriate models, is needed if one is to solve problems inherent in middle number systems. CEA problems cannot be resolved with the reductionist, analytical approach used for small number systems or with the statistical approach used for large number systems.

Fundamentally new conceptualization and methodologies need to be developed for CEA based on a more complicated form of causality than we are accustomed to using. Cause and effect and especially the causality emanating from feedback relationships is fundamental in CEA. For example, qualitative structures of causal inter-relationships of biophysical, socio-economic and management links are needed. Meaningful linking of the various disciplines is critical for successful CEA. Our impression is that the classical errors in managing whole systems have been made by ignoring interconnection, or not understanding the dynamics, of the whole. These were not errors in the detailed evaluation of individual links. In fact, traditional methods of analysis usually determine the effects of individual links in a system very well, but poorly determine the scope of a system to be studied, or how the parts fit together in the larger system.

2. Conducting a cumulative effects assessment is not "doing science". Although successful assessment is often very dependent on a good scientific information base, CEA and EIA are processes needed mainly for decision making. Thus, conducting a CEA or an EIA is not equivalent to "doing science". In many ways, the scientific method is a limited one. It is based on a reductionist thinking mode that has sometimes been carried to the absurd. Weinberg (1975) likened current science to a tool box. It has some very useful tools but they are not all good for all types of problems. Some of the recent attempts to make environmental assessment "scientific" have laudable motivations but are based on incorrect assumptions. Preston and Bedford (1987) in their recent review of CEA give the impression we should be attempting rigid experimentation, hypothesis-testing and quantification of statistical significance. For the most part, these are not even applicable concepts in CEA. To illustrate this point we describe two examples: the first illustrates the inappropriate use of the experimental approach in EIA and the second, the incorrect application of hypothesis-testing.

Example 1

Often a project or action is viewed as an experiment (National Research Council, 1986). The "experimental approach" in an assessment context is fraught with pitfalls and could even be described harshly as an approach which circumvents long-term environmental management goals. Lehman (1986) cited a case where a detailed South Indian Lake Impoundment study was conducted and later researchers examined long-term (and largely unanticipated) impacts in a post-study evaluation. The level of this lake was raised in a large-scale diversion of the Churchill River to supply hydroelectric power. A large number of impacts, many of which could be considered cumulative, resulted from the development. In particular, some of the cumulative effects with water quality and mercury contamination of fish were not anticipated.

This failure of EIA in the project-as-experiment approach caused the loss of the largest commercial freshwater fishery in northern Manitoba. Total catch eventually fell to one-third of its pre-impoundment size as operators abandoned the fishery (Lehman, 1986). The environmental consequences, and socio-economic impacts on the northern Native fishing industry, were severe and compensation-legal claims have already exceeded \$2 million. These facts outline the perils of the "experimental" approach and point out the hidden

costs which may be entailed by not striving for a CEA-type approach in major project decisions. The Native Peoples of South Indian Lake region suffered health risks and economic impacts. They did not even share in the scientific adventure. This project is a clear demonstration of the ethical and regulatory need to develop CEA methodologies in Canada and to ensure that they are properly applied.

A fundamental problem with CEA is the extreme complexity of assessments which may involve several projects affecting numerous environmental components over large time and geographic scales (Peterson *et al.*, 1987). Because ecosystems are large and complex, it is usually impossible to do rigorous scientific experiments with valid experimental controls. Exceptions to this include the work accomplished at the Experimental Lakes Area by the Freshwater Institute and the long-term ecosystem studies at Hubbard Brook, New Hampshire. In the first example, valid experimental (whole-lake) manipulations have been accomplished with significant scientific benefits. These cases are rare in North America, but they provide rationale for long-term commitments to ecological field studies by institutions with significant scientific capabilities. Laboratory results concerning ecological processes can rarely be extrapolated with predictive, scientific confidence to field situations. As Lane and Levins (1977) concluded, physiological truth in the laboratory can equate to ecological myth in the field.

Example 2

A second example of applying the scientific method in an inappropriate way for CEAs is advocating hypothesis generation and testing for impact predictions. Hypothesis-testing is often recommended in Canadian EIAs, usually to be implemented in subsequent monitoring programs. For example, the Beaufort Sea Environmental Monitoring Programme (BEMP) centers upon generation of hypotheses, and testing them is central to the conceptual framework of BEMP.

Experimentation in ecological research generally leads to the application of inferential statistics in order to demonstrate treatment effects resulting from the experimental perturbation or alteration. The use of inferential statistics entails many explicit and implicit assumptions concerning experimental design, sampling, data frame, and the analysis and interpretation of the resulting data. A particularly important prerequisite for inferential statistics is true replication of treatments which many ecological studies and EIAs fail to provide (Hurlbert, 1984). For example, often studies which purport to satisfy this requirement in fact do not and thus commit what Hurlbert (1984) terms "pseudoreplication". This term can be defined as "the use of inferential statistics to test for treatment effects with data from experiments where either treatments are not replicated, though samples may be, or replicates are not statistically independent." Thus, pseudoreplication is replication of samples at some level other than the treatment unit, for example, blocks, sub-samples, etc.

While formulation of testable hypotheses and development of sound experimental designs suitable for statistical analysis are desirable goals, in practice they are not often achieved and the concept is not applicable to many assessment and monitoring situations. Major problems exist, then, in

determining environmental change associated with anthropogenic activities for which inferential statistics can and cannot be applied and in developing new approaches to analyze existing and future data sets where inferential statistics are not appropriate. Almost sixty years ago, R.A. Fisher, the father of modern statistics, stated: "No one would now dream of testing a response to a treatment by comparing two plots, one treated and the other untreated" (Fisher and Wishart, 1930).

Yet, we continue to design expensive and even sophisticated assessments to generate hypotheses and monitoring programs to test these hypotheses that cannot be scrutinized statistically. Fisher developed inferential statistics for application to questions of experimental design. It is of vital importance to the EIA process in Canada to ensure not only that rigorous statistical methods are used where appropriate, but that other criteria be developed to consider those instances in which traditional statistical methods are not appropriate. This will obviate the subjection of well-intentioned assessments to criticism and invalidation on inappropriate statistical assumptions. The Experimental Lakes and Hubbard Brook Studies did not use inferential statistics to "test" for change.

3. Several steps in traditional environmental impact assessment have direct counterparts in cumulative effects assessment. The impacts and changes inherent in cumulative effects are not fundamentally different from those involved in more traditional types of environmental assessment in that all changes must eventually occur in a local ecosystem whether or not the ecosystem is predominately urban or wilderness. Changes can involve loss of reproductive potential of a species leading to local extinction, or decrease in water clarity leading to a decline in primary production in a lake. The "tyranny of small decisions" also teaches us that cumulative effects do not need to be profound locally; in fact, they can be very small. Thus, it is logical that many of the steps and analytical tools used in EIA will also be used in CEA. What is fundamentally different, however, between CEA and EIA, is the larger scale and often, complex pattern inherent in CEA as compared to EIA. The complexity of the pattern often arises from feedback relationships of cause and effect among the environmental components that occur on extended scales of space and time. For some CEA steps, such as bounding, only the scales of space and time substantively change; yet for other steps, fundamentally-different methods are needed such as cause and effect modeling. In the analyses of spatially-extended ecosystems, it is the indirect and community effects which often assume major importance in developing predictive assessments (Appendix 8.2). Hence, the theoretical requirements for fundamental advances in CEA demands that a more holistic approach to pattern be undertaken in contrast to EIA.

4. Cumulative effects assessment should be thought of as a process, not a method per se. CEA is a framework for linking the conceptualization of diverse types of information on fairly broad scales, with a central theme of cause and effect. Much of the U.S. work in CEA is now aimed at developing a process, not a particular method. As Appendix 7.3 demonstrates, the U.S. has completed a large number of case studies and has a large number of separate agencies working on CEA. Much of the CEA activity in the U.S. was motivated by specific references to cumulative effects in U.S. legislation. In Canada,

there has been less formalized on CEA in regulations than in the U.S. Although case studies can be tremendously informative, there needs to be some organized efforts to develop the conceptual approach and process with associated analytical tools that are common to most case studies. Thus, there should be parallel and integrated development of the overall process, analytical methods and applications (case studies) if Canada is to have an operable approach to CEAM. No one part can substitute for the other two.

The cumulative effects assessment process must include a menu of analytical methods. In most cases, a combination of these methods will be needed. There is an inherent need for modeling and conceptualization in developing the framework for CEA. Modeling is inherent to all complex system problems, and there is a large array of model types from mental constructs, to physical miniatures like a wind tunnel, to large computer simulation models. Therefore, a valence concept is one necessary criterion for choosing a method: not just how well does it perform the task intended, but how easily can it be linked to other methods needed to complete other tasks in the CEA process. Some case studies end up like "models by committee", and are too cumbersome to achieve the goal of an operable CEA process. Likewise, development of a process abstracted from practical application can be meaningless. What is desirable, as an end product, is a flexible cumulative effects process that encompasses a menu of methods. What is not always appreciated is that some methods are interchangeable in their basic mathematical form. For example, matrices and networks can be used to represent the same set of information. Whereas there is often no difference in content, there is a difference in the presentation of information and the immediate use and understanding of it by the user.

5. Cumulative effects assessment must allow for the appropriate linkages among the various disciplines and institutions. The same concept of valence may be applied to institutional collaboration. Mechanisms are needed to provide valence among participants at more than one organizational level, and for cooperation both vertically and horizontally in the institutional and jurisdictional structure.

One way to look at CEA is through linkages. Ignorance on the part of the user with respect to links among causes as well as links from causes to effects is problematic: for example, links that operate ecologically at different spatial and temporal scales than our institutions can manage can yield an accumulation of impacts. New links are also needed between disciplines to combat the ignorance that accumulates through linked and increasingly complex anthropogenic activities.

Some cumulative effects are the result of links between economic policy and objectives on one hand and ecological policy and objectives on the other. These links carry effects in both directions. We tend to ignore these (cross-disciplinary) links in economic and environmental analyses, which are usually carried out separately. We probably cannot afford that separation if we want to understand and manage CE. Central to this separation of links is often a basic jurisdictional or institutional problem.

Many conferences and papers have led to recommendations for changed, usually more centralized, larger and more controlling, public institutions. Such recommendations should be made modestly, and implemented carefully. What might the cumulative effect of many strengthened institutions be on our somewhat fragile, unique and seeming successful, political and economic systems? Strong, centralized political systems in other parts of the world have not always led to enhanced environmental quality or higher standards of living.

Institutions are very powerful and important in producing both ecological and economic "cumulative effects". Changes in institutions should be approached with awareness and caution precisely because they are so powerful and recalcitrant. This leads to a policy prescription that matches Holling's recommendation that institutional change should follow improved ecological knowledge but perhaps for a different reason. Holling (1982) seemed to believe that environmental understanding is the more powerful force for change and that institutions will arise to implement the "new ideas". More to the point is the understanding of the institutions involved; and institutional change can be adaptive and promote improved ecological understanding.

Of course, the real chain of events will be iterative, but we need to recognize that institutions are both causes and consequences of ignorance regarding environmental problems and cumulative effects. We need to accumulate knowledge about both if we are to solve our CE problems. It is not that institutions should await better ecological understanding or precede it, but that institutions and their interaction (links) with ecology and economics must be better understood and managed. Whereas a planning focus is needed. North American countries do not have strong central planning in their history or immediate future. Therefore, we should seek to accomplish planning objectives with modest institutional adjustments.

3.0 A FRAMEWORK FOR CUMULATIVE EFFECTS ASSESSMENT

Building upon the assumptions discussed in Section 2.4, a framework for CEA has been developed that involves the user first deciding whether or not there is a cumulative effects problem. Note that one of the major decision points is whether or not the impacts are expected to extend beyond the local ecosystem. We have assumed that one of the key CE distinguishing criteria separating CEA from EIA is the concept of the larger space and time scales inherent in CEA. Second, if there is a cumulative effects problem, the type of causality it entails must be identified. The subsequent ordering of the steps that should be followed in the CEA process is outlined. For each step, references are given to associated methods.

3.1 Is A Cumulative Effects Assessment Needed?

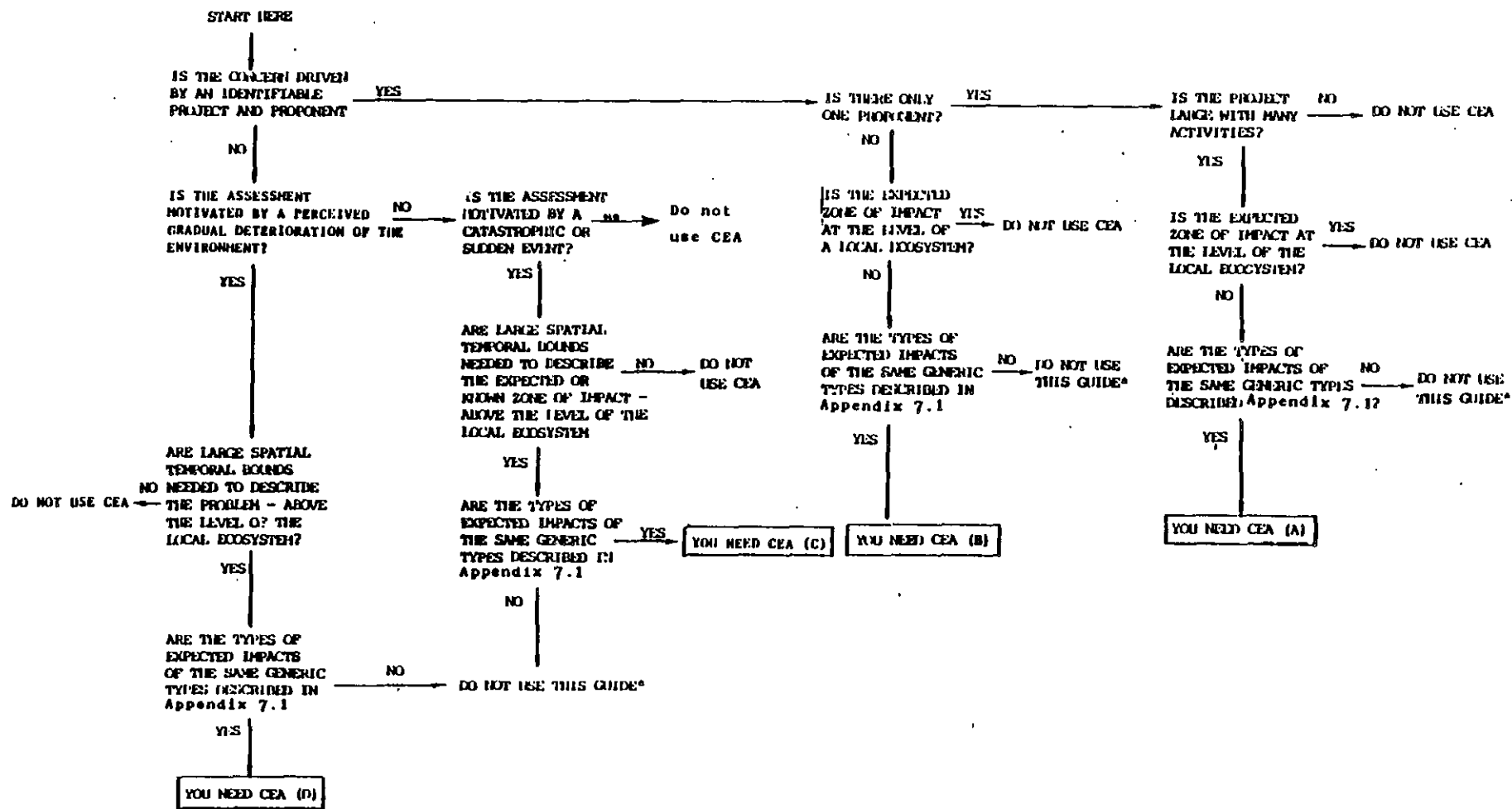
Use the decision tree outlined in Figure 3.1 for determining whether or not there is a cumulative assessment problem and whether or not this Guide will help to resolve it. The ends of the branches culminate in three basic kinds of decision: (1) Yes, use this Guide and the type of CEA problem is identified as Type A, B, C, D; (2) No, do not use this Guide; and (3) Perhaps this Guide will be useful. In the third case, there is a borderline example and it must be studied further before passing through the decision tree again. Often, careful attention paid to Chapter 3.0 will help clarify whether or not borderline cases can be resolved using this Guide.

In Figure 3.2, the basic characteristics of the four types (A-D) of cumulative effects are described. For each type, the underlying mechanism can be periodic (time crowding), synergistic (space crowding) or combined (both) as per the definition of CEs in Chapter 1.0.

The Type A cumulative effects problem involves a large project that has multiple activities and is proponent-driven. An example is the proposed expansion of the low level flying of NATO forces centered at Goose Bay, Labrador. The ecology must be studied at a regional level because the airplanes potentially affect an area much larger than the local ecosystem. Noise and acidifying emissions have been predicted to impact wildlife, aquatic resources, water quality and native peoples over large areas. With this type of CEA, (Type A), one would expect the impacts to radiate outward from the focus of the project, the Goose Bay airfield, as it undergoes major construction and operation activities during the expansion. The potentially affected impacted area is larger than many countries, and cumulative effects have already been observed resulting from the interaction of jet emissions and long-range transport of atmospheric pollutants (LRTAP). A FEARO panel has been convened to review the environmental impact statement (EIS) for this development.

In the Type B CEA, we are concerned with multiple projects usually associated with multiple activities. The projects may or may not be of the same kind. With this type of CEA problem, impacts radiate out from the individual project foci and can interact with each other, resulting in a diffuse and often complex spatial pattern. Major waterways, estuaries and the Great Lakes provide a multitude of examples of Type B CEA problems. In this type of CEA,

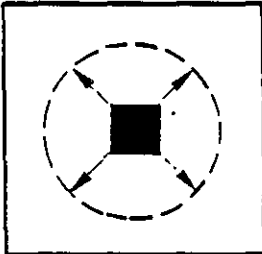
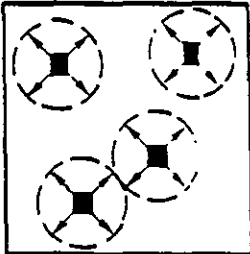
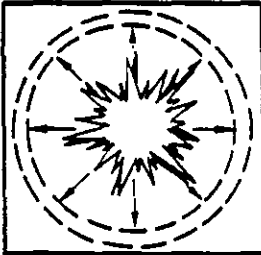
Figure 3.1 Decision Tree for Answering the Question: Is a Cumulative Effects Assessment Needed?



* You may have a cumulative effects assessment requirement but you will need to be careful in evaluating whether or not your concern is close enough to the generic ones listed in Appendix 7.1 for this Guide to be applicable.

Figure 3.2 Basic Characteristics of the Four Types (A,B,C,D) of Cumulative Effects (CE). For Each Type (A-D), Cumulative Effects can be Periodic, Synergistic or Combined.

Basic Spatial Pattern

<u>Primary Motivation</u>	Identifiable Focus (Inductive)	Diffuse Pattern (Deductive)
Proponent Driven	Type A 1 Large Project Multiple Activities Cause of Perturbation 	Type B Multiple Projects Multiple Activities Cause of Perturbation 
Ecosystem Driven (No Identifiable Proponent)	Type C Catastrophic or Sudden Event (natural or anthropogenic origin) Cause of Perturbation 	Type D Broad Scale Environmental Deterioration (primary cause can be many small activities to one large one) Cause of Perturbation 

the same proponent may have been involved in more than one traditional assessment. Even if there have been multiple assessments of individual projects with the same proponent, however, the inter-relationships among the assessments have not been considered.

Type C and Type D CEAs lack identifiable proponents. These types of CEA are termed: "ecosystem-driven". In Type C, there has been a sudden and often catastrophic event. Examples include a large fire or other catastrophe where the causality is obvious. Perhaps the origin of the fire is not known but that the ecological destruction was related to a major fire is obvious. Another example occurred in Cape Breton, Nova Scotia, where spruce budworm populations reached epidemic proportions and destroyed large expanses of forest. Although the immediate causal link was obvious (namely, that the spruce budworm destroyed the forest), the underlying cause and effect and the role of humans have been more elusive factors. Controversy remains on whether or not spraying is the preferred management option. History suggests there may be long time scales of the order of decades related to these insect outbreaks.

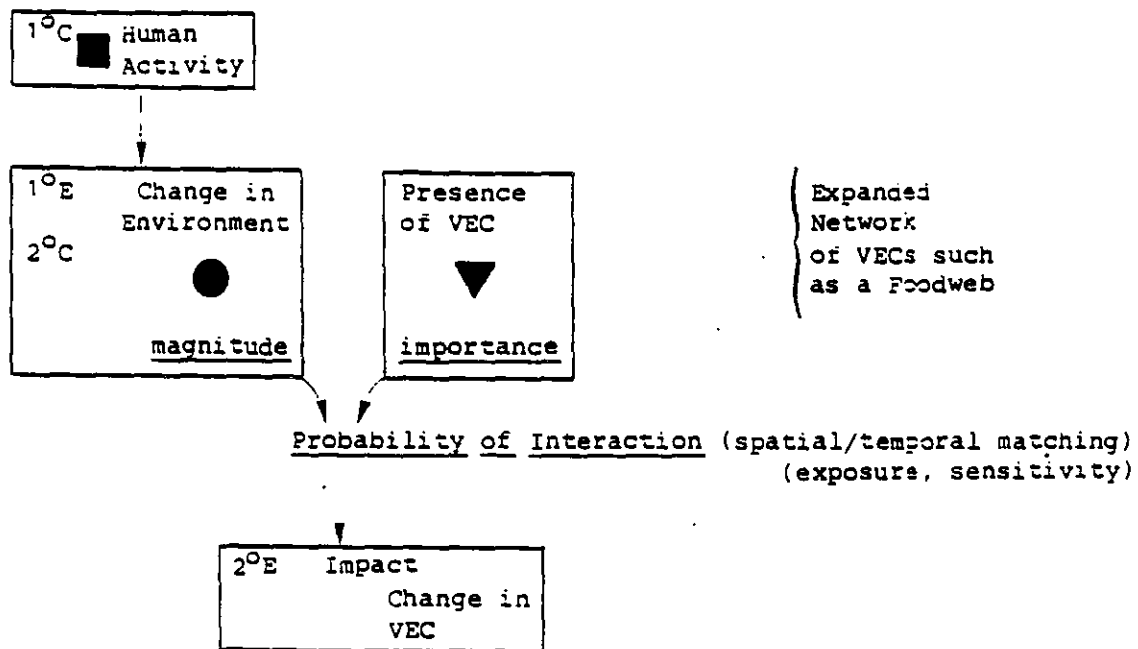
In Type D, there has been noticeable and broad-scale environmental deterioration. This deterioration might result from one or many human activities and developments, but usually the causality is unknown. In identifying the acidic deposition problem, for example, it was initially noticed on a large regional scale that many lakes that had supported fish populations in the past no longer supported fish, or that if fish existed in these environments, species diversity was much reduced and particular species had become locally extinct. It was later discovered that the fish were dying or failing to reproduce because of the low pH of the lake water which was subsequently linked to atmospheric emissions from industrial sources in central Canada and the U.S. As with the "proponent-driven" types of CEA, the "ecosystem-driven" types include very large spatial and temporal scales involving both an identifiable focus (Type C) and a diffuse pattern (Type D).

As with any characterization or typology of a set of complex problems, this separation of all CEA problems into four basic types is oversimplified. The typology holds for a large number of examples, but there will always be intermediate types. This typology, however, gives the assessor an initial point to identify and begin a cumulative effects assessment. As mentioned earlier, the fundamental tenets of cumulative effects assessment are: (1) that the causality is complex, and yet, it must be unraveled in an understandable way, and (2) the spatial-temporal scales and patterns are extended beyond that of the local ecosystem. Most assessors are not trained to understand causality on such extended spatial-temporal scales. In the next section, we build upon the four basic types of CEA given above and describe the basic networks of cause and effect associated with each type.

3.2 Characterizing the Type of Causality

In Figure 3.3, the conceptualization of cause and effect in traditional environmental impact assessment is illustrated. First, there is some sort of human activity and/or development which is termed the primary cause (1^0C). This activity causes a primary change (1^0E) in the environment. This change

Figure 3.3 Generalized Model of Environmental Impact.



Key:

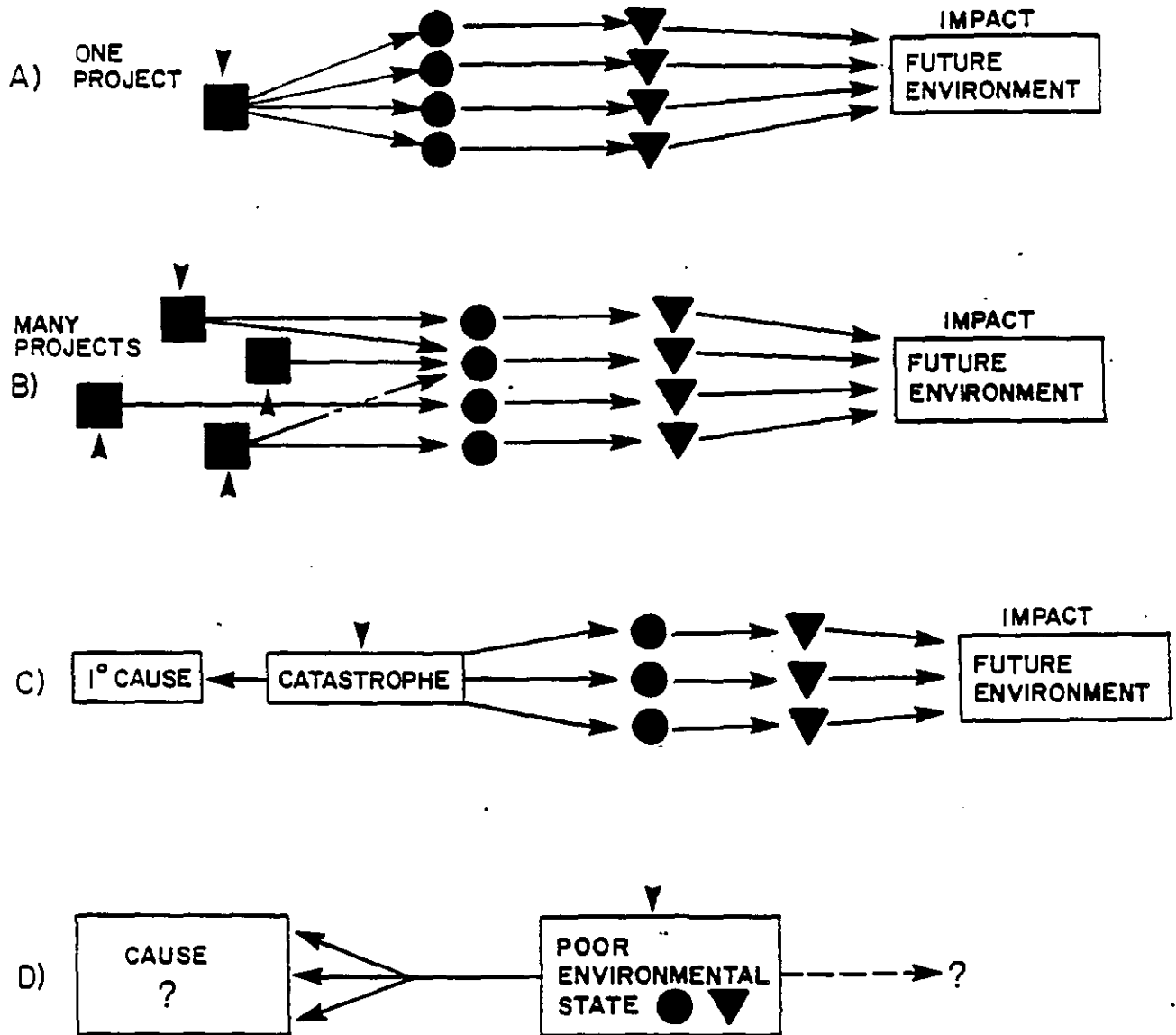
- 1° C = first degree or primary cause
- 1° E = first degree or primary change
- 2° C = second degree or secondary cause
- 2° E = second effect or secondary change
- VEC = Valued Ecosystem Component

in the environment, in turn, becomes a secondary cause ($2^{\circ}C$) when it interacts with a Valued Ecosystem Component (VEC) to cause a secondary effect ($2^{\circ}E$) which is an impacted VEC. In order for there to be an impact, there must be a change in the environment which interacts with a Valued Ecosystem Component to a sufficient degree to change the VEC in either a positive or negative way. [Note: throughout the Guide, we use the term Valued Ecosystem Component to indicate a broader range of environmental components (both biophysical and socio-economic) than intended by the originators of the concept (Beanlands and Duinker, 1983)]. They defined VECs as those components that would be identified in a social scoping. In the Guide, we include all components that have intrinsic value to humans, ecosystem integrity, etc.)

The causality portrayed in the model in Figure 3.3 is the simplest that can be described for traditional impact assessment, which is usually proponent-driven. Obviously, there is also some artificiality involved in this impact model. The delineation of the environmental changes from VECs is a human construct, not one of nature; thus, what might be a minor change in the environment to one assessor might be a major shift in a VEC to another assessor. In traditional impact assessment, however, this basic model has been used repeatedly. In some EIAs the impacted VEC is predicted without showing the intervening steps in the causal chain. In other assessments, a matrix might be used giving numerical scores to magnitude, importance and probability of impact. The degree to which the VEC will be affected by a change in the environment will depend on the overlap of spatial and temporal bounds, the degree of exposure of the VEC, and its sensitivity to the particular environmental change. Note that in this model, the causality is diagrammed as one-way and sequential. In an ecological system, the VEC and the environmental change would, themselves, be part of a more complicated form of causality. The VEC might be an important species, such as a salmon, which would be a component of a food web. Likewise, human activities are often interrelated in complex causal webs. To understand causality in ecological systems, it is always necessary to use models to simplify the essence of cause and effect. The key problem is to capture the essence of causality with a minimum of links. Lane (1986a,b) and Lane and Wright (1986) described how this could be done for a coastal marine community undergoing multiple perturbations (Appendix 8.2).

In Figure 3.4, each of the basic types of cumulative effects assessment problems are diagrammed using the reasoning in Figure 3.3. For each CEA Type (A-D) there are environmental changes and Valued Ecosystem Components, however, there are differences in the basic patterns of causality. For Types A and B, the flow of causality is from the left to the right of the page. For Type C, causality goes in both directions. The assessor may wish to deduce the primary cause of the catastrophe or predict the future effects. In Type D, most of the causality flows from the right to the left of the page. Essentially, when we reason from the specific to the general, or the parts to the whole, we are using inductive reasoning as shown for Types A and B. When we reason from the general to the specific, we employ deductive reasoning such as shown in Type D. Type C is an example of both inductive and deductive thinking since causality flows in both directions. Insofar as possible, all four types of CEA are illustrated using simple straight line causality.

Figure 3.4 Basic Types of Causality in Cumulative Effects Assessments. Types A and B Employ Inductive Reasoning and Type D Uses Deductive Reasoning. Type C Involves Both Inductive and Deductive Reasoning. ▼ Indicates where Assessment Begins.



KEY:



HUMAN ACTIVITY

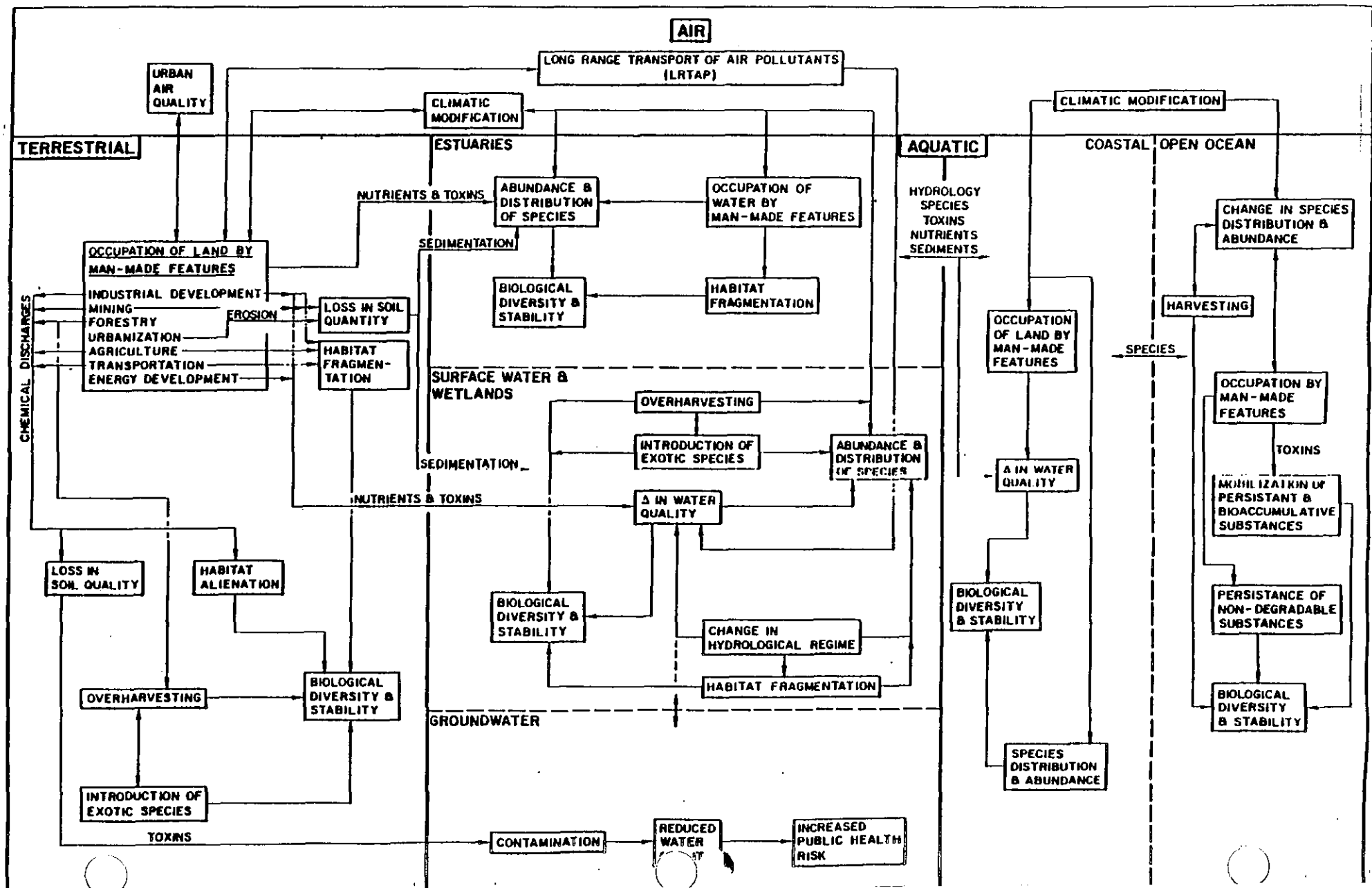


CHANGE IN THE ENVIRONMENT



VALUED ECOSYSTEM COMPONENT (VEC)

Figure 3.5 Interrelationships of Major Cumulative Effects Problems by Ecosystems as Identified in Appendix 7.1.

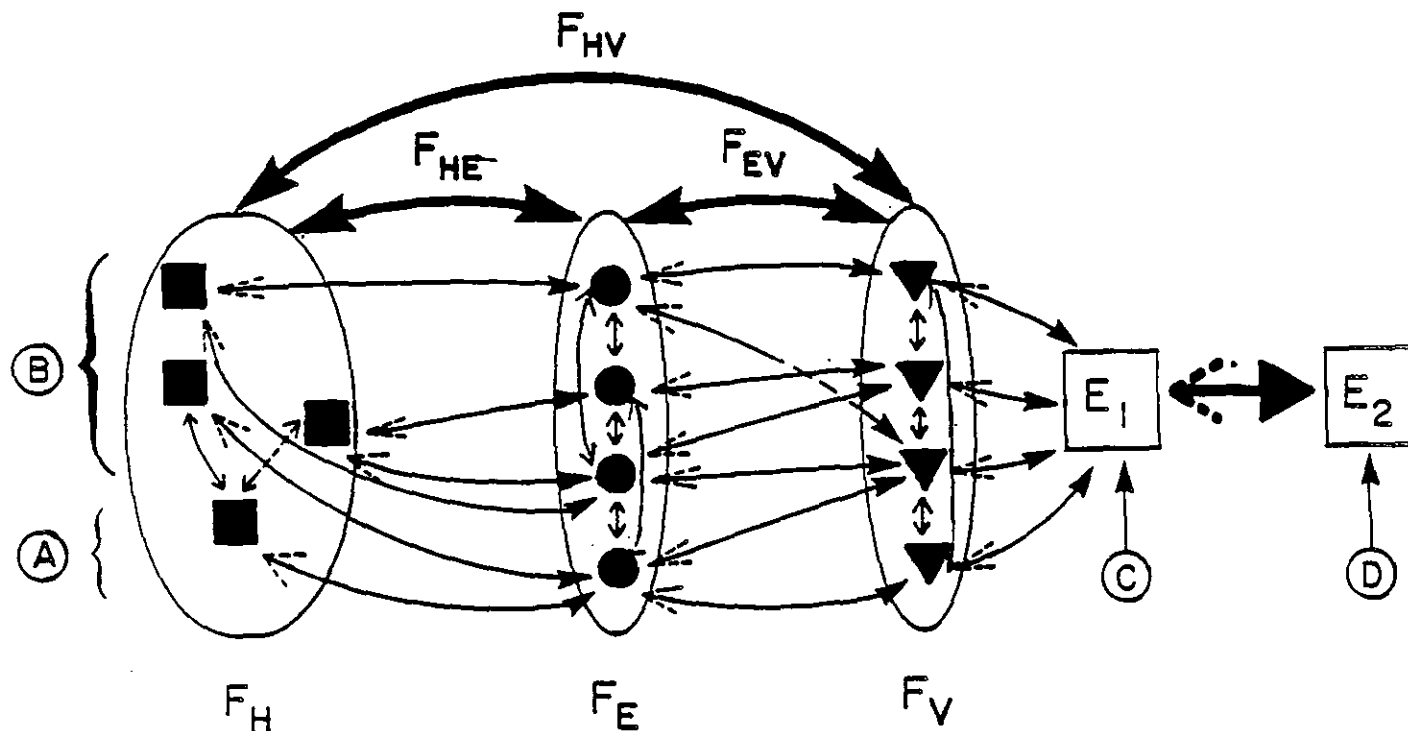


True causality in the natural world is seldom this simple. First, for any particular set of boundaries we would like to use in a CEA, there is probably more than one cumulative effects problem occurring. In Figure 3.5, the basic kinds of CE problem (described by region in Canada, in Appendix 7.1), are shown by interrelated ecosystems. Using this diagram, it is clear that a large number of CE problems potentially occur in the same space and time and contribute to overall environmental deterioration. If one chooses to complete an assessment only for area X on the acidic deposition problem, one may delete the causal links related to the Valued Ecosystem Components associated with other CE problems. Under particular circumstances this action is legitimate. For example, the deleted links might be considered to have a minimal effect, or logistical support might not be available for a larger assessment. It is always preferable, however, that the CE assessor carefully note what is and what is not being included in the assessment and determine if an omission will affect the final conclusions of the assessments.

In Figure 3.6, all four types of CEA (diagrammed in Figure 3.4) are combined into a single figure. It becomes clear that where the assessment begins in the pattern of causality differs with the four types of CEA problems. In Types A and B, the assessor starts with the human activities and developments and works forward to predict what will happen in the future. In Types C and D, the assessor works backward to determine the causes of the present state of the environment. Illustrated also in this figure are the locations of potential feedback (F) in the causal network. Feedback may be in terms of human activities (H), environmental changes (E), and Valued Ecosystem Components (V).

For example, F_H might arise from the interaction of the marketplace driven by the profit motive and an electric company's goal to supply cheap power. The company does not install scrubbers and its power plants release tons of acidified atmospheric pollutants (NO_x and SO_x). These emissions lead to changes in aquatic environments and initially a lowering of pH in lakes and rivers. Through F_E , the lowered pH values change the solubility conditions for heavy metals such as aluminum. This causes aluminum to go into solution and become available to various groups of aquatic organisms. At high concentrations, aluminum is toxic to the organisms and can compound the existing stress related to the acidity. One of the VECs in this environment might be a sport fish species. Because of the change in pH and aluminum toxicity, competitive relationships might change among fish species in the foodweb. Lowered reproductive success of the VEC of interest, and concurrent, higher predation on its young from another fish species that was competitively superior to the sport fish would constitute F_V relationships. Indeed, most foodwebs have strong feedback relationships with and without the perturbations related to cumulative effects. As explained in the CEA framework assumptions (Section 2.4), the ecological mechanisms of the impact eventually operate at the level of the local ecosystem and can involve even lower levels of biological organization such as physiological, and behavioral and biochemical levels. What is different with acidic deposition, as compared to heavy metal toxicity or excess acidity shown for a single lake, is its large-scale spatial pattern which encompasses a large number of individual lakes.

Figure 3.6 Summary of Causality and Feedback for All Four Types (A-D) of Cumulative Effects. Solid Arrowheads Indicate Direction of Inductive Reasoning and Dotted Arrowheads Show the Direction of Deductive Reasoning.



In addition to the feedback within human activities or environmental changes or VEC's, there are also feedback relationships between pairs of these components. Environmental managers might act to regulate a particular type of human activity or development as soon as environmental change is observed and before the VECs undergo a negative impact. Likewise, environmental changes can lead to changes in VECs that, in turn, change their behavior or physiological patterns which, in turn, lead to subsequent ecological change. When a VEC is noticeably affected, however, is usually the time when environmental managers influence human activities so as to lessen their negative consequences. These feedback links may be one- or two-way. They represent an important class of control mechanisms and options that might be available to the manager and regulator.

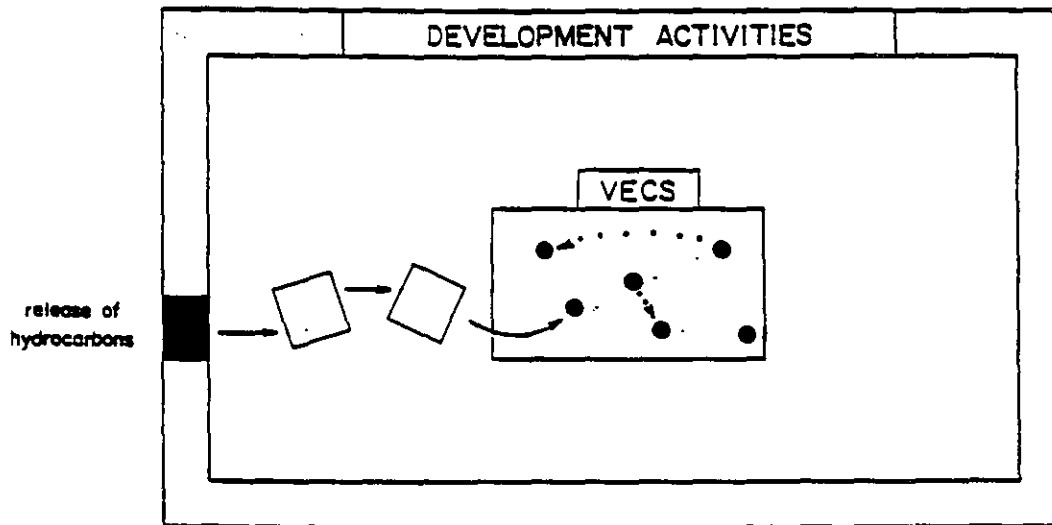
The concept of feedback and hence interconnection at extended spatial scales is the quintessential concept in cumulative effects assessment. The essence of conducting good cumulative effects assessments is to identify the locations and strengths of the key feedback relationships at the appropriate scale. This is not easy to do, and no precise prescription can be written that will guarantee success for every CEA. Much of the complexity, however, in CE is bound up in these extended feedback relationships. It is important to note that cumulative effects themselves are not fundamentally different from those effects that occur in local ecosystems: reproductive capacity may be diminished, feeding behavior may be altered, individuals may die, a lake may acidify, profit may decrease, a way of life might change irreversibly, or a social value may be lost. What is different are the patterns of these changes on a regional scale over longer periods of time. In addition, since many of these impacts are cumulative in a multiplicative way, there may be irreversible or sudden environmental damage before the environmental managers have time to respond appropriately. This is coupled with the fact that small changes locally, that are usually ignored, may have severe cumulative and regional consequences.

Failure to account for feedback even in its simplest form is evident in the way the Beaufort Environmental Monitoring Program (BEMP) was conceptualized (Figure 3.7). BEMP represents one of the most organized efforts to conduct assessment and develop rational monitoring plans on a regional level in Canada. In many ways, it has been a precedent-setting process. Although BEMP has been criticized for employing untestable hypotheses as the central theoretical construct in the program, BEMP can also be criticized for employing causal explanations that are too simplistic in framing hypotheses. Overall, BEMP is equivalent to a Type B cumulative effects problem. Multiple activities mostly related to oil and gas production in the Beaufort Sea are hypothesized through inductive reasoning to affect on a set of VECs. Approximately twenty hypotheses have been framed and are in use today.

In Figure 3.7, the BEMP conceptualization is compared to one that more realistically includes feedbacks. Most of the VECs identified are populations of aquatic organisms or higher organisms: mammals, birds and fish. All are interrelated in the aquatic foodweb. A set of hypotheses that predicts changes in isolated VECs that are in reality intimately interconnected cannot succeed. We know from network theory that even a single feedback loop

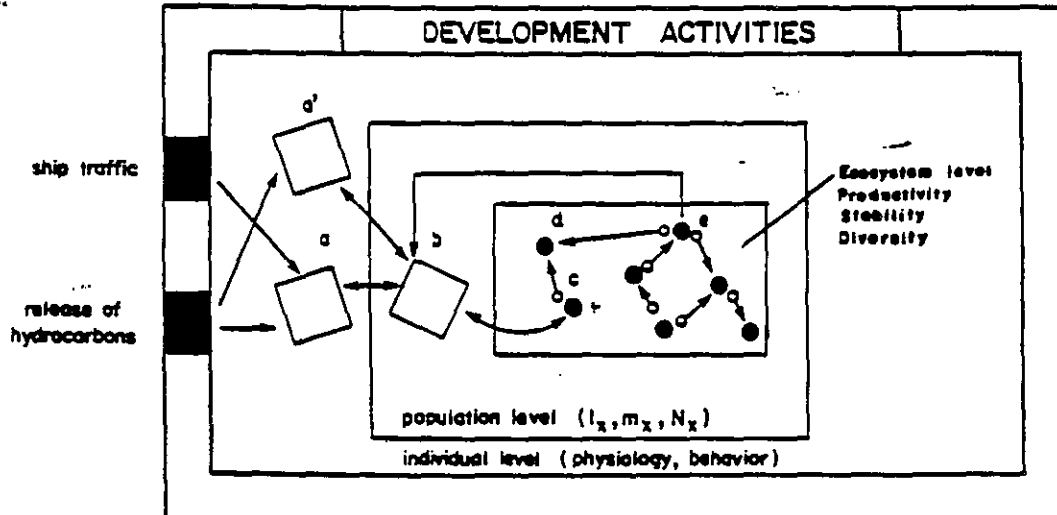
Figure 3.7 Comparison of BEMP Conceptual Approach to an Integrated One Centering Upon Feedback and the Ecological Heirarchy.

BEMP APPROACH



CEA INTEGRATED APPROACH

EXTENDED CAUSE & EFFECT
INDIRECT EFFECTS
FEEDBACK AT MULTIPLE LEVELS



KEY:

- - DEVELOPMENT ACTIVITY
- - VALUED ECOSYSTEM COMPONENT (VEC)
- - CAUSAL STEP

FOR THE INTEGRATED APPROACH:

- a - INDIVIDUAL LEVEL
- b - POPULATION LEVEL
- c - ECOSYSTEM LEVEL

strategically positioned in a network of cause and effect can lead to unexpected and even counterintuitive results (Lane and Levins, 1977).

Most environmental managers have an intuitive grasp of the counter-intuitive aspects of ecological systems. For example, pesticides kill insects in laboratory bottles but often fail to control them in nature. Why is this? It is not because we have observed an incorrect physiological response in the laboratory but, rather, the diverse feedbacks in ecological systems can result in an increase in pests where the physiology predicts a decrease. There are many ways this could happen. For example, the pesticide may have killed the predators which are slower growing than the prey pest of interest. Released from predation, the pest species may have increased in numbers even though some individuals were killed by the pesticide. Likewise, when nutrients enter a lake, there may be large blue-green algal blooms and subsequently, lowered concentrations of nutrients. In this case, the turnover rate of the nutrient pool has increased greatly whereas its concentration observed at a single time, has decreased. None of these ecological observations negate the physiology upon which they are based. In a complex system with feedback, however, many physiological truths become ecological myths (Lane and Levins, 1977).

These examples illustrate the importance of feedback in environmental systems. They also demonstrate that the choice of a suitable level of organization and scale to study an environmental observation or to predict impacts is all-important. An unsuitable choice leads to great difficulty in characterizing the minimal causality needed for understanding and predictive accuracy. Just as the physiological level is not appropriate to understand the local ecosystem, the local ecosystem is not adequate to understand events at a regional level. This is not to say that physiological information (in the first case) and local ecosystem understanding (in the second case) are not useful. Such understanding is not only useful but often necessary. What is emphasized, however, is that information at lower levels of organization and scale is often not sufficient to explain behavior at higher levels. If one knew everything about chemistry and physics it would still not be possible to write the equation for life. The essence of organization of living systems is not wholly contained within the sciences of chemistry and physics. Fundamentally different types of system behavior, that are not totally subsumed and apparent at lower levels, emerge at higher levels in a hierarchy.

3.3 Steps in the CEA Process

Because of the complexity of most CEA problems and because there are so many separate activities that must be conducted to complete a CEA, no single set of methods can be presented as the method for "doing cumulative effects assessment". More realistically, CEA should be thought of as a process of several steps. For each step, there may be several specific methods that could be employed. There is no single right method for any particular step. The process varies in the organization and ordering of steps for the four types of CEA. It is possible to discriminate two processes: one where the goal of the CEA is mostly a bottom-up, inductive reasoning process (Types A and B); the other where it is a top-down, largely deductive reasoning process (Types C and D). Note Types A-D were illustrated in Figure 3.2. The major

steps for the bottom-up process are given in Table 3.1 and for the top-down process in Table 3.2. It is assumed before using either Table 3.1 or 3.2, that Figure 3.1, the CEA decision tree, has been used and the assessor has reached one of the four terminal branches that states that the CEA Reference Guide for Type A, E, C, or D should be used. If any other terminal branches are reached, the assessor should not proceed in this Guide.

Step 5 in Table 3.1 and Step 6 in Table 3.2 relate the CEA diagram illustrated in Figure 3.6. Because this diagram summarizes all four types (A-D) of CEA problems, the diagram appears more complicated than it is. For types A and B use only the solid arrows, and for types C and D the dotted arrows. By constructing the diagram system understanding can be focused regardless of whether there is a single or several assessors. Alternative diagrams can be used if there is disagreement on the key causal links.

A detailed discussion of the other steps is not given here. Many are self-evident and logical extensions of steps used in traditional environmental impact assessment. In Volume II (Wallace and Lane, 1988) the cumulative effects problem of the prairie wetlands is described using the decision tree. It is a Type D problem and we illustrate how the CEA process can be applied. This concrete example is more helpful to the assessor than a hypothetical discussion of the steps. Since both the bottom-up and top-down processes have many steps in common, a single example such as the prairie wetlands can be used to illustrate both processes.

Both inductive and deductive thinking is used in understanding the natural world: neither mode is superior to the other. Scientists, for example, routinely use both. As a gross generalization, however, these changes that occur in the environment that are basically chemical (toxic chemical pollution in the Great Lakes) will necessitate a more complex set of feedback relationships for conceptualization than will those changes that are largely physical such as habitat fragmentation. Likewise, often those changes that are mainly of a physical nature can be indicated placed on maps more successfully than can chemical changes.

For the bottom-up process, where there is an identifiable proponent, development or set of developments, the initial scoping of Step 1 and the bounding of Step 2 could be greatly facilitated if there were formal arrangements for CEA in the federal regulatory and assessment practices. In the recommendations given in Chapter 5.0, we suggest that there should be at least five CEA boards, one for each region of Canada. If such boards were formed, it is envisaged that they would have changing membership as the need arose, but the core membership would represent the best expertise available on the key CE problems of a particular region as well as the necessary blend of jurisdictional and institutional interests. One of the roles of the board would be to identify key cumulative effects problems and set regional environmental standards and monitoring programmes to ensure that CE were being monitored, managed and predicted adequately. Thus, there would be certain goals established by region and a system of environmental planning initiated. Jurisdictional and institutional arrangements would need to be adjusted to accommodate this initiative. When proponents wished to begin a new

development, it could be screened against the backdrop of regional cumulative effects.

If there is not to be a formalized system nor identified regional goals for environmental quality and resource management, then it will be exceedingly difficult to have proponents account adequately for cumulative effects. First, it might not be in their interest to do so; and second, it would be difficult for them to have a sufficient regional overview and CE understanding to complete a satisfactory CEA even if they were motivated to do so. Usually for a proponent, the goal is to pass the regulatory hurdles and to pose no direct threat to the environment in spatial-temporal dimensions which are as small as and as circumscribed as possible. Other human activities impinging on that bounded area are given little direct consideration except insofar as they form part of the environmental description or if there is some direct way in which they might impede the development (both construction and operation). This process could be radically altered and improved with an integrated regional environmental planning and CEA process in effect. If there is not to be a formalized approach that integrates regional environmental planning and assessment capabilities successfully, it would appear that the best that could be achieved is a reactive approach that hastily organizes an ad hoc solution whenever a particular cumulative effect problem becomes intolerable.

Even disregarding the environmental damage that will result from continuing our present reactive approach, it is probable that if long-term economic indicators could be employed they would show the true economic cost related to CEs, then the political unit could be better motivated to combat CEs.

3.4 Selecting the Analytical Methods

For both the top-down and bottom-up CEA process, a selection of analytical methods for each step is given in Table 3.3. For some steps, there is only a single method listed; for other steps, several methods are given. Often the selection of a method will depend upon the logistical base available to the assessor. For many steps, in either the top-down or bottom-up process, a workshop or other collective format can be used to implement the method. In other steps, a single assessor might work alone. In Table 3.4, the logistical base using criteria of budget, data set, and intensity of assessment effort, is contrasted for space crowding (synergistic), time crowding (periodic) and combined types of CEA problems. In each category, the preferred methods are directly related to the degree of feedback in the system requiring characterization. For low feedback CE problems, usually less elaborate and less costly methods are needed. Loop analysis is the most appropriate method of characterizing high feedback CE problems. The method centers on the characterization of feedbacks and cause-effect pathways. If there are few data, then hypothetical loop models (Type 1) should be used; and if there is an appropriate data base, Type 2 models are recommended. An ecosystem 20-25 variables can contain thousands of pathways of effect and many hundreds of feedbacks. We know of no other method than enumerates these pathways and feedbacks in terms of ecologically meaningful predictions.

Table 3.5 contains a listing of the methods used in CEA by basic type. Relevant examples of each type are described in Appendix 8.1. Loop analysis

is described individually in Appendix 8.2, because it is presently the best method for characterizing ecological feedback and it is not widely understood although there are approximately 40-50 references to it in the literature. In addition, several methods listed in Table 3.4 were developed in the United States and are also described in Appendix 7.3. For consensus building methods, see Section 4.3. References to all methods are listed in Chapter 6.0, Bibliography.

Table 3.1 Steps in a CEA for the Bottom-Up Process Needed for CEA Types A, B and Some Portions of C.

Figure 3.1 should be used to determine type of CEA problem before using this Table.

STEP	ACTIVITY
1	Scoping (define questions, issues, potential detail of analysis, CEA goals and logistical support).
2	Bounding (define universe of human activity, potential environmental change, VECs, institutional and jurisdictional boundaries).
3	Go through Figures 3.2 - 3.5 to establish common basis of understanding.
4	List human activities, environmental changes, and VECs.
5	Prepare a forward CEA diagram (solid arrows) in Figure 3.6.
6	Decide on the location and amount of feedback (F_H , F_E , F_V , F_{HE} , F_{HV} , F_{EV}).
7	Decide on the CEA problem as one of space crowding (synergistic), time crowding (periodic), or combined (Table 3.4).
8	Select the analytical tools and perform the analysis to determine the predicted state(s) of the environment (Tables 3.3 and 3.4). (Characterize also the level of uncertainty associated with the predictions.)
9	Diagram predicted qualitative states.
10	Decide if the future states are acceptable, have potential to be mitigated, or are not acceptable and a way to alter causality in CEA diagram must be found.
11	Explore management options, design strategy, and make recommendations (including additional data collection, more sophisticated analysis, environmental effects monitoring, post project audits, socio-economic adjustments, institutional and jurisdictional adjustments, etc).
12	Repeat steps 4-12 if additional scenarios of potential human activities are hypothesized to occur in the future and decisions need to be made concerning equitable division of the regional ecosystem.

Table 3.2 Steps in a CEA for The Top-Down Process for CEA Types C and D.

Figure 3.1 should be used to determine type of CEA problem before using this Table. (Note: If you have a CEA Type C problem and the cause of the sudden event or catastrophe is well understood, you will need only the inductive approach described in Table 3.1.)

STEP	ACTIVITY
1	Scoping (define questions, issues, potential detail of analysis, CEA goals and logistical support).
2	Bounding (define spatial-temporal universe of concern for observed environmental deterioration, institutional and jurisdictional boundaries).
3	Go through Figures 3.2 - 3.5 to establish common basis of understanding.
4	Diagram known changes in qualitative state of human activities, environmental changes, and VECs.
5	List unacceptable and worrisome trends.
6	Prepare a backward CEA Diagram (dotted arrows in Figure 3.6).
7	Identify all human activities that should be included in the diagram and establish hypothetical links.
8	Decide on the location and amount of feedback (F_H , F_E , F_V , F_{HE} , F_{HV} , F_{EV}).
9	Decide on the CEA problem as one of space crowding (synergistic), time crowding (periodic), or combined (Table 3.4)
10	Select the analytical tool and perform the analysis to determine the causes of the observed environmental deterioration (Tables 3.3 and 3.4). (Characterize also the level of uncertainty associated with the causes.)
11	Explore management options, design strategy, and make recommendations (including additional data collection, more sophisticated analysis, environmental effects monitoring, postproject audits, socio-economic adjustments, institutional and jurisdictional adjustments).
12	If you have a Type D CEA problem assume corrective adjustments and prepare a forward CEA Diagram to project whether future states of the environment are predicted to be acceptable or not.
13	Repeat steps 7-13 if additional scenarios of potential human activities are hypothesized to occur in the future and if decisions need to be made concerning equitable division of the regional ecosystem.

Table 3.3 Some Suggested (Types) of Methods For Each Step in the CEA Process: Bottom-Up Approach, and Top-Down Approach. (Refer to Appendix 8.1 for a description of each type of method.)

Bottom-Up Approach

Step	Method
1	<u>ad hoc</u>
2	mapping, overlays, some simple models and calculations
3	use existing guide matrices and networks (Figures 3.2 - 3.4)
4	checklists
5	network (using existing guide format, Figure 3.5)
6	<u>ad hoc</u> networks, loop analysis
7	mapping, overlays, graphical methods, trend analysis
8	<u>ad hoc</u> mapping matrix, loop analysis, computer simulation
9	mapping, overlays, graphical methods, trend analysis
10	<u>ad hoc</u>
11	<u>ad hoc</u> , consensus building
12	same as steps 4-12 above

Top-Down Approach

Step	Method
1	<u>ad hoc</u>
2	mapping, overlays, some simple models and calculations
3	use existing guide matrices and networks (Figures 3.2 - 3.4)
4	checklists
5	checklists
6	network (using existing guide format, Figure 3.5)
7	checklist and network
8	<u>ad hoc</u> networks, loop analysis
9	mapping overlays, graphical methods, trend analysis
10	<u>ad hoc</u> , mapping matrix, loop analysis, computer simulation
11	<u>ad hoc</u> , concensus building
12	network (using existing guide format, Figure 3.5)
13	same as steps 7-13 above

Table 3.4 Selection of an Analytical Tool.

Once the CEA diagram is constructed as shown in Figure 3.6, levels of low and high feedback can be determined by the amount of interconnection in the diagram.

Logistical Base				
	Short Term Effort, Low Budget, Small Data Set		Long Term Effort, High Budget, Large Data Set	
	Type A & B -----	Type C & D -----	Type A & B -----	Type C & D -----
1. <u>Space Crowding</u>	(ad hoc)	(ad hoc)		
Low Feedback	mapping cause/effect	mapping back step analysis cause/effect	geographic information system modeling	geographic information system modeling
High Feedback	loop analysis* (Type 1)	loop analysis (Type 1)	loop analysis (Type 2) computer simu- lation	loop analysis (Type 2) computer simu- lation
2. <u>Time Crowding</u>	(ad hoc)	(ad hoc)		
Low Feedback	cause/effect	trend analysis back step analysis cause/effect	forecasting risk analysis modeling	forecasting risk analysis modeling
High Feedback	loop analysis (Type 1)	loop analysis (Type 1)	loop analysis (Type 2) computer simu- lation	loop analysis (Type 2) computer simu- lation
3. <u>Combined</u>	(ad hoc)	(ad hoc)		
Low Feedback	trend analysis mapping cause/effect	back step analysis mapping trend analysis cause/effect	risk analysis modeling	risk analysis modeling
High Feedback	loop analysis (Type 1)	loop analysis (Type 1)	loop analysis (Type 2) computer simu- lation	loop analysis (Type 2) computer simu- lation

*Loop Analysis Types 1 and 2 are explained in Appendix 8.2

Table 3.5 Summary of References (1-9) Used in Appendix 8.1, Plus References for Three Additional Categories: Loop Analysis, Trend and Forecasting, and Risk Analysis Methods (10-12). References are listed in Chapter 6.0.

Method	Reference
1) <u>ad hoc</u>	- California Energy Commission, 1982. - Colorado Department of Health and U.S. Environmental Protection Agency, 1981. - Denver Research Institute and Resource Planning Associates, 1979. - Finsterbusch, K., 1977. - Hirst, S.M., 1984a,b. - Maryland Department of Natural Resources, 1982. - Reed, R.M., J.W. Webb and G.F. Cada, 1984. - Roy F. Weston, Inc., 1978.
2) <u>checklists</u>	- Armour, C.L., R.J. Fisher and J.W. Terrell, 1984 - Battelle Columbus Laboratories and Midwest Research Institute, 1979. - Bloom, S.A., 1980. - Canter, L.W., 1981. - Center for Wetland Resources, 1977. - Contant, C.K. and L. Ortalano, 1985. - Dames and Moore, Inc., 1981. - Dee, N., J.K. Baker, N.L. Drobny, K.M. Duke and D.C. Hanringer, 1972. - Everett, S.J., 1978. - Geppert, R.R., C.W. Lorenz and A.G. Larson, 1984. - Hydropower Assessment Steering Committee, 1983. - INTASA, 1981a,b. - Leopold, L.A., F.E. Clark, B.R. Hanshaw, and J.R. Balsley, 1971. - Mason, W.T., Jr., 1979. - Oscar, Larson, and Associates, no date. - Sassman, R.W. and R.M. Randall, 1977. - U.S. Army Corps of Engineers, 1980.
3) <u>matrices</u>	- Bloom, S.A., 1980. - Canter, L.W., 1981. - Contant, C.K. and L. Ortalano, 1985. - Dames and Moore, Inc., 1981. - Gilliland, M.W. and B.D. Clark, 1981. - INTASA, 1981a,b.

Table 3.5 (cont'd.)

Method	Reference
3) matrices (cont'd.)	- Kane, J., I. Vertinsky and W. Thompson, 1973. - Leopold, L.A., F.E. Clark, B.B. Hanshaw, and J.R. Balsley, 1971. - Roy F. Weston, Inc., 1978. - Sorenson, J.C., 1971. - Streeter, R., R. Moore, J.J. Skinner, S.G. Martin, T.L. Terrel, W. Klimstra, J.J. Tate and M.J. Nolde, 1979. - Yorke, T.A., 1978.
4) network	- Armour, C.L., R.J. Fisher and J.W. Terrell, 1984. - Armour, C.L., 1986a,b. - Bain, M.B., J.S. Irving, R.D. Olsen, E.A. Stull and G.W. Witmer, 1985a,b,c. - Canter, L.W., 1981. - Caswell, H., 1976. - Coullard, D., 1984. - Gilliland, M.W. and B.D. Clark, 1981. - Sorenson, J.C., 1971.
5) overlays	- Colorado Department of Health and U.S. Environmental Protection Agency Region VIII, 1981. - Dickert, T.G. and A.E. Tuttle, 1985 - Fabos, J.G., C.M. Greene and S.A. Joyner, Jr., 1978. - Kimball, T.C., A. Patel and G.A. Yoshioka, 1982. - Lumb, A.M., 1982a,b. - McHarg, I., 1969. - U.S. Department of Interior, Bureau of Land Management, 1977. - Winn, D.S. and K.R. Barber, 1985.
6) modeling procedures	- Boreman, J., C.P. Goodyear and S.W. Christensen, 1978. - Canter, B., 1986. - Caswell, H., 1976. - Center for Wetland Resources, 1977. - Coats, R.N. and T.O. Miller, 1981. - Contant, C.K. and L. Ortalano, 1985. - Darnell, R.M., 1973. - Everett, S.J., 1978. - Federal Energy Regulatory Commission, 1984.

Table 3.5 (cont'd.)

Method	Reference
6) modeling procedures (cont'd.)	- Geppert, R.R., C.W. Lorenz and A.G. Larson, 1984. - Gilliland, M.W. and B.D. Clark, 1981. - Hinds, W.T., 1984. - Hirst, S.M. 1984a,b. - INTASA, 1981a,b. - Kane, J., I. Vertinsky and W. Thompson, 1973. - Kozlowski, J., 1985. - Phillips, Brandt, Reddick-McDonald and Grete, Inc., 1978. - Sanderson, D. and M. Ottare, 1977. - Schneider, D.M., D.R. Godschalk and N. Axler, 1978. - Shaw, S.H., 1982. - Slatyer, R.O. (ed.), 1977. - Tahoe Regional Planning Agency, 1982. - U.S. Department of Agriculture, Forest Service, 1981. - U.S. Army Corps of Engineers, 1983.
7) evaluation techniques	- Anderson, B.F., 1981. - Anderson, D.M. and C.C. King, 1978. - Armour, C.L., 1986a. - California Energy Commission, 1982. - Capen, D.E., 1981. - Denver Research Institute and Resource Planning Associates, 1979. - Finsterbusch, K., 1977. - Haines, Y.Y. and A. Ha. 1974. - Keeney, R.L. and H. Raiffa, 1976. - Lichfield, N., P. Kettle and M. Whitbread, 1975. - McAllister, D.M., 1980. - Moreau, D.H., E.L. Hyman, B. Stiftel and R. Nichols, 1981. - Phillips, Brandt, Reddick-McDonald and Grete, Inc., 1978. - Tahoe Regional Planning Agency, 1982. - U.S. Fish and Wildlife Service, 1981.

Table 3.5 (cont'd.)

Method	Reference
8) adaptive methods	- Anderson, D.M. and C.C. King, 1978. - Beanlands, G.E. and P.N. Duinker, 1984. - Cole, C.A. and R.L. Smith, 1983. - Dames and Moore, Inc., 1981. - Dee, N., J.K. Baker, N.L. Drobny, K.M. Duke and D.C. Hanringer, 1972. - Denver Research Institute and Resource Planning Institute, 1979. - Dickert, T.G. and A.E. Tuttle, 1985. - ESSA, 1982. - Everitt, R.R., 1983. - Holling, C.S. (ed.), 1978a. - Horak, G.C., E.C. Vlachos and E.W. Cline, 1983a,b. - Jameson, D.T. (ed.), 1976. - McAllister, D.M., 1980. - New Mexico Hydrology Task Force, 1984 - Oscar, Larson, and Associates. no date. - Thomas, M., F. Hopkins and R. Parshal, 1982. - U.S. Department of the Interior, Bureau of Land Management, 1977. - Vincent, M., 1981. - Walters, C.J., 1986. - Warner, M.L., J.L. Moore, S. Chatleriee, D.C. Cooper, C. Ifeadi, W.T. Lamhow and R.S. Reimers, 1974. - Williams, D.C. and K.B. Hom, 1979.
9) general references	- Bain, M.B., J.T. Finn, L.J. Gerardi, Jr., M.R. Moss and W.P. Saunders, Jr., 1982. - Beanlands, G.E., 1986. - CEARC and U.S. NRC (eds.), 1986. - California State Board of Forestry, 1982. - Canter, L.W., 1977. - Erikson, P.A., 1979. - Galloway, G.E., 1978. - Jeffrey, M.I., 1987. - Morrill, R.A., 1973. - Sondheim, M.W., 1978. - USFW, 1983. - Williamson, S., 1986. - Witmer, G.W., 1985a,b.
10) loop analysis	- Lane, P.A., 1982 - Lane, P.A., 1985a,b. - Lane, P.A. and J.A. Wright, 1986. - Lane, P.A. and R. Levins, 1977.

Table 3.5 (cont'd.)

Method	Reference
11) trend analysis and forecasting	- Makridakis, S. and S. Wheelwright, 1978.
12) risk analysis	- Suter, G.W., L.W. Barnthouse, C.F. Baes, S.M. Bartell, M.G. Cavendish, R.H. Gardner, R.V. O'Neill and A.E. Rosen, 1984. - ESSA, 1985. - O'Neill <u>et al.</u>, 1982.

4.0 CONCLUSIONS AND RECOMMENDATIONS FOR CEAM IN CANADA

Several conclusions and recommendations for the general CEA development in Canada are grouped below in three categories:

- 1) Development of CEA Approach and Process,
- 2) Jurisdictional and Institutional Concerns, and
- 3) Consensus Building.

4.1 Development of a CEA Approval and Process

Conclusion:

Developing a practical CEAM approach and process and finding ways to implement CEAM may be the single best way to preserve environmental quality in Canada. It is feasible for Canada to develop its own CEA process and conceptual framework as described in this Guide. The ordering of the steps in the process relate largely to the CEA assessor's point of entry in the CE cause and effect network. The Guide essentially provides the broad outline of a practical CEA process; there is still much to do to supply the user with detailed, custom-designed methodologies for particular steps in the process. It is important that the process, individual methods and case studies (applications) develop in an integrated fashion. It would be desirable to develop a collaborative CE methodology with U.S. workers.

Recommendations:

- 1) Case Studies: There should be at least two case studies implemented that would typify the top-down and bottom-up CEA process outlined in the Guide. The wetlands of the boreal agricultural fringe of the Prairie Provinces problem discussed here as a Feasibility Study in Volume II (Wallace and Lane, 1988) is a top-down (ecosystem-driven) CEA problem with little ecological feedback through its dominant features of habitat fragmentation, hunting and natural predation, spatial patchiness and physical alteration. There are, however, some key feedback relationships at the socio-economic and institutional levels. In this study, population risk modeling would facilitate the meaningful integration of the diverse pieces of ecological data which could then be combined with causal network analysis of the socio-economic, institutional and jurisdictional valuables.

The second case study should be a bottom-up (single to multiple) proponent-driven CEA problem that involves an important aquatic resource. This example should be characterized by chemical pollution which would involve ecotoxicological effects throughout the biological hierarchy (individual, population and ecosystem) resulting in diverse ecological feedbacks.

It is important for both case studies, that multiple user groups are identified and that there is enough public interest involved to warrant

giving major attention to the problem. One possibility for the aquatic study would be Georges Bank where there is presently a great deal of controversy surrounding hydrocarbon exploration. This example also has interesting jurisdictional and institutional ramifications. Another worthwhile example would be the plight of the Atlantic salmon in the Maritime Provinces in regard to acid deposition and other impacts.

- 2) Methodological Development: Intense development is needed to understand how causality and feedback operate in spatially and temporally extended regional ecosystems and to detail this understanding in a practical form for users. If we cannot learn to identify the strong feedbacks and basic qualitative structures of these systems, we will always be at the mercy of Murphy's Law (anything that can go wrong, will go wrong) and its corollary (the thing that can do the worst damage, will inevitably happen first). Murphy's Law is the downside of middle number system problems. Murphy's Law is applicable to many areas of human endeavor, and human activities within ecosystems have provided a long list of examples supporting its validity.

The basic qualitative structure must include the human activities, the environmental changes, the VECs and their feedback within and between these sets of components. In addition, the biophysical part of the structure cannot and should not be separated from the socio-economic, jurisdictional and institutional components. Key areas of feedback, points of natural and human control, as well as basic links may influence the dynamics of the system being assessed. A well-intentioned and even reasonable management edict positioned in a particular part of the network can actually lead to the deterioration rather than the improvement of the ecosystem or species the edict is meant to protect.

The main causal network technique (loop analysis) that is recommended is based upon feedback relationships among a set of interacting variables. Many other types of qualitative techniques can only represent one-way causality and thus much of the feedback needed to understand CE is ignored. Although loop analysis has been mainly developed for local ecosystem applications, its use should be extended to CE-type problems. Strongly quantitative modeling approaches should only be implemented after the qualitative structure and level of feedback are understood.

Once developed, loop models can be transformed into computer simulation models by a competent modeler. Loop analysis models have an advantage in that they can be used with and without data. Variables need not be in the same units. An environmental manager or a habitat protection law can be placed in the same network occupied by a caribou population without converting everything to grams of carbon or kilowatts of energy. These models can be used by the mathematically unsophisticated, and they are inexpensive. Loop models are also very helpful in guiding data collection and framing impact predictions. Most of the other methods listed in Chapter 3.0 need only minor revisions to be used in the CEA process.

- 3) Integration of Regional Planning and Assessment Capabilities: At present, regional environmental planning and environmental assessment capabilities are usually not possessed by a single individual to a sufficient degree to achieve successful integration of the two subject areas. Planners and ecologists often have very different academic backgrounds and types of professional experience. There needs to be a concerted effort to develop training programs for CEAM that are either totally new or that augment existing environmental studies programs in Canada. In addition, workshops are needed preferably focused upon case studies that will facilitate the integration of planners and assessment specialists. Results from these workshops and other collaborative efforts can be used to develop curriculum materials for a variety of purposes related to training and education for CEAM.
- 4) Logistical Considerations: First, a software package for guiding a user through the CEA process and the basic qualitative modeling should be developed. Second, there should be improved and more organized information exchange between the developers of the CEA process in the United States and Canada. We have much to learn from each other.

4.2 Jurisdictional and Institutional Concerns

Conclusion:

The jurisdictional and institutional systems in Canada are not adequate to deal with the multitude of CEA problems identified in Chapter 3.0 and Appendix 7.1. Even when the regional ecology is well understood, the problem is not always solvable because of the jurisdictional and institutional barriers. The acid deposition problem is a good example of this. Any meaningful CEA process must contain steps that guide a user through a typology of these barriers and a set of options for dealing with them. This was outside the Terms of Reference of the present Reference Guide. An assessor needs to be able to identify the feedback associated with jurisdictional and institutional concerns that might enter the dynamics and qualitative structure of the ecosystem under assessment. New arrangements for CEAM should be explored at the federal and provincial levels.

Recommendations:

- 1) Methodological Development: A separate study should be commissioned to develop the above typology and study potential feedbacks of jurisdictional and institutional actions with the environment including both its socio-economic and bio-physical components. Recommendations should be made to modify the Canadian CEA process to include steps to guide the user through the myriad of potential problems and management options. These recommendations should be tested in the case studies and improved as necessary.
- 2) New Arrangements: One reason that the U.S. workers have placed much more emphasis and effort on CEAM than have their Canadian counterparts is the fact that the U.S. included cumulative effects in key legislation

early in their EIA history. Although Canada may be behind the U.S. in formal efforts, we are still ahead in that we have fewer people, smaller CE problems, less bureaucracy and jurisdictional and institutional barriers than does the U.S. We should, however, strengthen and extend our current assessment and planning approach to ensure that CEs do not slip through undetected and unabated.

One way to achieve a CEAM process in Canada would be for FEARO to assume a coordinating role with five regional CEAM assessment boards for each of the five geographical regions of Canada (Atlantic Provinces, Quebec, Ontario, Prairie Provinces, and British Columbia and the Territories). The boards would have both federal and provincial representation. A modified form of the Regional Coordinating and Screening Committee might serve as a starting point.

At the federal level, there should be requirements that all proponents have their IEEs and EISs screened against a set of cumulative effects guidelines. P. Lane and Associates Limited/Washburn and Gillis and Associates Limited recently included a chapter on cumulative impact assessment for the Generic Initial Environmental Evaluation for the fixed link proposed between New Brunswick and Prince Edward Island (Lane and Gillis, 1988). Twelve potential areas with cumulative effects were considered and analyzed. The FEARO coordinating committee for cumulative effects would need to develop the necessary training materials to implement proponent-driven CEA.

For ecosystem-driven CEA, each CEAM assessment board should serve as an early warning system for detecting CEs near threshold values. These boards could be a focal point for regional system description, data collection and monitoring, goal-oriented management objectives, identification of real or potential CEs, and recommendations for management, planning, and research. Generic templates and software packages as well as GIS systems could be developed as needed for all five CEA areas to facilitate the implementation of standardized CEA practices throughout Canada.

It is unlikely, especially for CEs arising from generalized ecosystem deterioration, that any single government agency (and certainly not proponents) will assume responsibility for the management of CEs if FEARO does not assume a lead role in developing and implementing new CEAM requirements.

4.3 Consensus Building

Conclusion:

Consensus building is an important part of successful CEAM. Consensus building is needed in several of the steps in the CEA process described in Chapter 3.0 such as setting of objectives and delimiting boundaries, agreeing on the monitoring of impacts and actions to be taken when thresholds are exceeded, and formulating new institutional and jurisdictional arrangements to ensure CEAM is operable in Canada.

Achievement of consensus among parties-at-interest involved in environmental disputes or negotiations has become an increasingly important objective for government agencies, and, in some cases, for proponents. In cumulative effects assessment, the decision makers often must determine the total carrying capacity of the environment for development and other human activity. This necessitates the resolution of multiple-user conflicts and the optimal allocation of environment among these users.

O'Riordan (1983) documented approaches to the consultative process as part of strategic planning for regional water resources in British Columbia. The use of public advisory groups as a component of the early strategic planning process was cited as a method to minimize future disputes. More recently, McGlennon and Susskind (1987) noted that the USEPA, finding 80% of all its new regulations being challenged in court, initiated a demonstration project to test the usefulness of negotiated rule-making.

These two examples at the regional planning stage and in the development of regulations, demonstrate the potential for application of consensus-building techniques across a broad spectrum of environmental issues. Jeffery (1987) explored the role and value of negotiation and mediation within the existing assessment/approval process in Ontario. The author contended that the negotiation/mediation process should not be allowed to reduce the authority of the regulatory/approval process set out in existing legislation and suggested that the most desirable role for this process is at the pre-hearing consultation stage.

This view is interesting, as it suggests a parallel, or complementary, role for mediation as part of the existing regulatory process. Certainly, this has been the experience of the Alberta Energy Resources Conservation Board in seeking to resolve environmental issues between Syncrude and the Fort McKay Indian Band. In this latter case, there have been continuous negotiations carried out to identify, analyze and resolve long-term environmental issues, including cumulative effects.

The legal process, albeit somewhat confrontational, nonetheless constitutes a form of consensus-building, one with a defined (often binding) outcome. In Canada, the courts are less often used than in the U.S., because of the greater discretion embodied in our legislation and because the U.S. courts have usually taken a far more interventionist stance. The new Canadian Charter of Rights and Freedoms, however, may substantially alter this trend and may allow for more citizen input, through the legal process, into environmental regulation or decision-making. Sadler (1986a,b,c) noted:

"Environmental mediation and negotiated agreements are generally viewed as supplements to regulatory and administrative procedures, rather than as an alternative to them. An important consideration thus becomes how best to tie these approaches to existing systems, for example, as preconditions for general approval or as requirements for the issuance of specific terms and licenses as development proceeds."

These types of concerns may imply a substantive need for the development of consensus-building techniques in cumulative effects assessment, at several levels:

1) Inter-governmental consensus

Clearly, more than any other environmental issues, cumulative effects embrace large areas, many jurisdictions and long time periods. The need for cooperation among Canadian and international governments will be a key factor in achieving success in this area.

2) Intra-governmental consensus

As noted above, the cooperation of a broad range of agencies will be vital to the successful implementation of CEA approaches. This will dictate cooperation between not just those agencies involved in environmentally-related resource management but also those with widely-differing, but applicable mandates, such as Revenue/Taxation.

3) Public cooperation

In order for CEA to evolve beyond the assessment state to implementative programs, an unprecedented degree of cooperation on the part of the public-at-large may be required. For instance, reclamation and preservation of regional wetlands will require the support of private landowners if any rehabilitative programs are to be successfully achieved. Once again, this implies a need for new mechanisms to facilitate public cooperation while providing, or devising techniques to give public incentives to do so.

For the reasons outlined above, CEA will clearly require new attention to, and methods for, the achievement of consensus within and between jurisdictions, governments and the public. These new initiatives will require careful examination in future, and strategies will have to be devised to ensure the cooperative recognition of CEA problems and to develop programs aimed at addressing them.

Recommendation:

A study should be undertaken to document the array of consensus building methods already in use and then, using this information, a practical methodology should be integrated into the Canadian CEAM process.

There is a direct application to the CEA process presented in Chapter 3.0 through a set of what is termed group methods that have been used to build consensus in CEA by the U.S. Fish and Wildlife Service and other U.S. agencies involved in CEA. These include: nominal group technique, back step analysis and FAST diagramming which are briefly described in Appendix 7.3. For any subsequent development of consensus-building techniques, the U.S. experience provides invaluable information and guidance for evaluating a variety of techniques and examples of potential applicability to the Canadian CEAM process.

5.0 GLOSSARY OF ACRONYMS*

AADC	Alberta Agriculture Development Corporation
ACE	Army Corps of Engineers, U.S. Department of Defense
AEAM	Adaptive Environmental Assessment and Management
AECB	Atomic Energy Control Board
AERCB	Alberta Energy Resources Conservation Board
AFS	American Fisheries Society
ANL	Argonne National Laboratory
BEMP	Beaufort Sea Environmental Monitoring Programme
BLH	Bottom Lands Hardwoods
BPA	Bonneville Power Administration
CAETEP	Committee on Applications of Ecological Theory to Environmental Problems (U.S.)
CCREM	Canadian Council of Resource and Environment Ministers
CE(s)	Cumulative Effect(s)
C/E	Cause/Effect Analysis
CIE	Cause and Effect Analysis
CEA(s)	Cumulative Effects Assessment(s)
CEAM	Cumulative Effects Assessment and Management
CEARC	Canadian Environmental Assessment Council
CEQ	Council for Environmental Quality (U.S.)
CI(s)	Cumulative Impact(s)
CIA	Cumulative Impact Assessment
CIAP	Cluster Impact Assessment Process (FERC)
CIP	Cumulative Impact Process (FWS)
CI Project	Cumulative Impact Project (NEC/FWS)
CPS	Collaborative Problem Solving (FWS)
CPT	Computer Planning Tool
CWA	Clean Water Act (U.S.)
CWS	Canadian Wildlife Service
DFO	Department of Fisheries and Oceans
DIAND	Department of Indian Affairs and Northern Development
DU	Ducks Unlimited
EA	Environmental Assessment
EARP	Environmental Assessment Review Process
ECAR	Ecosystem Component at Risk
ECON B	Economic Benefit Analysis
ECON I	Economic Impact Analysis
EEM	Environmental Effects Monitoring
EIA	Environmental Impact Assessment
EIS	Environmental Impact Statement
EPA	Environmental Protection Agency (U.S.)
EPP	Environmental Protection Planning
EPS	Environmental Protection Service
ERA	Ecological Risk Assessment
FEARO	Federal Environmental Assessment Review Office
FERC	Federal Energy Regulatory Commission (U.S.)
FRES	Fraser River Estuary Study
FWS	Fish and Wildlife Service (U.S. Dept. of Interior)

*Glossary also includes acronyms for Volumes II and III.

GL	Great Lakes
GLIMS	Great Lakes Information Management System
IB	Island Biogeography
IEE	Initial Environmental Evaluation
IJC	International Joint Commission
LAC	Least Acceptable Change
LIR	Legislative, Institutional and Regulatory Entities
LRTAP	Long Range Transport Atmospheric Pollution
NAWMP	North American Waterfowl Management Plan
NEE	National Energy Board
NEC	National Ecology Center (U.S. FWS)
NEPA	National Environmental Protection Act (U.S.)
NMFS	National Marine Fisheries Service (U.S. Department of Commerce)
NPS	National Park Service (U.S. Department of Interior)
NRC	National Research Council (U.S.)
NWT	North West Territories
ORP	Optional Risk Procedure
OTCR	The Ohio, Tennessee and Cumberland Rivers
OTS	Office of Toxic Substances
OWMC	Ontario Waste Management Corporation
PAE	Polyaromatic hydrocarbons
PGLC	Pacific Gas and Electric Company (U.S. - California)
POS	Plan of Study
RES	Recreation Economic Study
R&D	Research and Development
SAB	Scientific Advisory Board (CEA User's Guide)
SRES	School for Research and Environmental Studies, Dalhousie Univ.
TUNS	Technical University of Nova Scotia
UMRBC	Upper Mississippi River Basin Commission
UMRCC	Upper Mississippi River Conservation Committee
USDA	United States Department of Agriculture
USDE	United States Department of Energy
USDI	United States Department of Interior
USDOD	United States Department of Defence
USFS	United States Forest Services
VEC	Valued Ecosystem Component

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7.0 APPENDIX ON CANADIAN/U.S. EXPERIENCE

7.1 Overview of Cumulative Effects Problems in Canada

Cumulative effects problems observe no jurisdictional boundaries. Pollutants such as airborne contaminants from industrial stacks in the United States do not stop at the Canadian border nor is it possible to stop aqueous discharges from industrial manufacturing and/or processing from crossing provincial or territorial boundaries. It is difficult, therefore, to compartmentalize many CE problems to specific provinces or territories. Some CE problems, however, can be described generally in Canada by geographic region and ecosystem type.

The purpose of this appendix is to give a broad overview of CE problems by geographical region in Canada; this appendix is not designed to present an exhaustive compilation of known and potential CE problems or to prioritize them. This review, however, presents enough detail on CE problems to enable the reader to visualize, in concrete terms, both the complexity of CE and the specific regional concerns of greatest immediate interest.

In the first section of this appendix, Canada is discussed in terms of the following five regions: (1) Atlantic Provinces (New Brunswick, Nova Scotia, Prince Edward Island and Newfoundland and Labrador), (2) Quebec, (3) Ontario, (4) Prairie Provinces (Alberta, Saskatchewan and Manitoba) and (5) British Columbia and the Territories. Interviews with environmental managers and EIA practitioners across Canada were useful in identifying specific cumulative effects problems on a regional basis. In addition, Peterson *et al.* (1987); GAIA, An Atlas of Planet Management (Myers, 1984); State of the Environment Report for Canada (Environment Canada, 1987a); and Environmental Quality in the Atlantic Region (Environment Canada, 1987b) were used. The latter is a recent update of the state of the environment in the Atlantic Provinces based on data collected by Environment Canada (1983). There were no other regional compendiums available.

7.1.1 Categories of CE Problems

Peterson *et al.* (1987) identified thirteen categories into which cumulative effects could be organized. This characterization is based on a review of a number of national and international papers and reports and professional experience. In addition to Peterson *et al.*'s basic typology, we list an additional three problems which can be considered relevant in a Canadian context (Table 7.1).

Regionally, cumulative effects problems differ depending on the overall intensity of human activity, and level and type of development occurring in each area. In the interviews, perception of cumulative effects problems differed for each region. Some CE problems, however, appear to be common to all regions of Canada. These include the mobilization of persistent and bioaccumulated substances and the long-term containment and disposal of toxic wastes. In addition to industrial and municipal discharge of toxic substances, metal mines dispose of an enormous quantity of solid wastes which may contain substances such as arsenic, cadmium, lead and mercury. Other CE problems such as the long-range transport of air pollutants affect only

Table 7.1 Cumulative Effects Problems (*) in Canada by Region.

CE PROBLEMS	REGION					
	Atlantic	Quebec	Ontario	Prairies	British Columbia	NWT and Yukon
1. Long-range transport of air pollutants and ecosystem acidification	*	*	*			
2. Urban air quality & airshed saturation					*	
3. Mobilization of persistent or bioaccumulated substances	*	*	*	*	*	*
4. Climatic modification	*	*	*	*	*	*
5. Occupation of land/and water by man-made features			*	*	*	
6. Habitat alienation (aquatic and terrestrial)	*		*	*	*	*
7. Habitat fragmentation (aquatic and terrestrial)	*		*	*	*	*
8a. Decreases in soil quality		*	*	*		
8b. Decreases in soil quantity		*	*	*		
9. Use of agricultural, silvicultural and horticultural chemicals	*	*	*	*		
10a. Reduction of groundwater supplies			*	*		
10b. Groundwater contamination		*		*		
11. Increased sediment, chemical & thermal loading of freshwater and marine habitats.		*	*			
12. Accelerating rates of renewable resource harvesting	*		*		*	
13. Long-term containment & disposal of toxic wastes	*	*	*	*	*	*
14. Activities and developments producing carcinogenic-teratogenic effects	*	?	?	?	?	?
15. Loss of biological diversity	*		*	*		
16. Change in hydrological regimes of major rivers/estuaries.	*	*	*	*	*	

certain regions in Canada. Acid precipitation, for example, is a major concern to Ontario, Quebec and the Atlantic Provinces, but is not yet a big problem for the western Provinces although "Arctic haze" has been reported for the Territories. In the Atlantic Region, many CE problems are related to marine activities and developments. In the Prairie Region, CE problems are related to forestry and agricultural activities. Habitat fragmentation is probably being experienced in all provinces to a limited extent but is most serious in the Prairie Region where wetlands are being destroyed for agricultural uses.

There have been a number of scenarios proposed for the effects on climate and the atmosphere of anthropogenic pollutants discharged into the atmosphere. The general consensus is that the increasing amounts of CO₂ being discharged into the atmosphere will result in global warming by the end of the next century. Although the modeling of a climate system is complex and not all the forcing functions are known or understood, a number of models have been used based on the effects of an increase in CO₂. Using this scenario the following impacts have been estimated assuming a doubling of atmospheric CO₂ concentration by the year 2085 (Healey and Wallace, 1987):

- 1) an increase in annual mean temperatures over Canada of about 2C° (ocean fringes) to 4C° (up to 6C° in Central Canada in winter),
- 2) a consequent reduced area and duration of snow and ice cover and arrival and disappearance of icepacks,
- 3) greater cloudiness as a result of the increase in oceanic surface temperature,
- 4) increases in the rate of evaporation,
- 5) a national increase of almost 150 mm in precipitation per year, but with marked changes in distribution,
- 6) changes in windspeed,
- 7) changes in vegetation growth rates and other characteristics and growing season,
- 8) increase in annual runoff rate at high latitudes, and
- 9) sea level rise (40-60 cm in the next century),

Many CE problems culminate in a loss of biological diversity. Projects, such as monocultural growing of forest tree species after clearcutting, introduction of exotic species (fish, mammals, plants) to an ecosystem, or major habitat changes that create unsuitable habitat for indigenous species, have the potential to affect adversely the distribution and numbers of species normally found in a particular habitat. These projects can have major biological and economic implications.

7.1.2 Some Regional Examples

In the following sections selected examples of regional CE problems are discussed to illustrate the diversity and extent of CE problems in Canada.

Atlantic Provinces

All of the CE problems listed in Section 7.1.1 affect the Atlantic Region with varying degrees of intensity. CE problems especially relate to: (1) long-range transport of air pollutants resulting in acidification of lakes and rivers, (2) some forestry and agricultural practices, and (3) the overharvesting of freshwater and marine fisheries. Being primarily rural, the provinces in the Atlantic Region have not yet been seriously affected by urban-based pollutants such as motor vehicle emissions or urban expansion. Exceptions to this include the polyaromatic hydrocarbon (PAH) laden air emissions and aquatic effluents in Sydney, Nova Scotia, and the extensive flooding in Saint John's, Newfoundland, as a result of urbanization of the Waterford River watershed. Resource-based activities, both terrestrial and aquatic, have been the primary sources of CE problems to date.

There are a number of "hot spots" in the Atlantic Region where there are pollutant sources contributing to cumulative effects. These are areas where industries are the sources of both aqueous discharges of heavy metals and toxic chemical discharges, and acidic or carcinogenic air emissions. In addition, there are a number of large-scale projects that have the potential to create cumulative effects in the marine environment. These projects include the tidal power project on the Annapolis River, the proposed fixed link between New Brunswick and Prince Edward Island, NATO low level flying and air base expansion in Goose Bay, and offshore oil and gas exploration off the coasts of Newfoundland and Nova Scotia.

Climatic modification, although not now considered to be a critical problem in the Atlantic Region, has the potential to be serious in the long term. Climatic modification of the marine system can, for example, result in a change in fish distribution and abundance. If the associated sea level rise is as high as one metre, this could drastically alter the shape of coastlines and inundate many communities and coastal facilities (P. Lane and Associates Limited, 1988).

Quebec

The population of the Province of Quebec is primarily concentrated in the south, especially along the St. Lawrence River. CE problems identified through interviews with environmental managers and practitioners included: (1) chemical pollution in the St. Lawrence River, (2) accumulation of acid mining residues, (3) fertilizer, insecticide and herbicide accumulations, (4) soil and groundwater contamination from forestry and agricultural practices, (5) soil erosion, (6) acid rain, and (7) ozone contamination. In addition, the problems which have been ranked as most critical in Quebec include the long-range transport of air pollutants and ecosystem acidification, climatic modification, and mobilization of persistent and bioaccumulated substances.

Five CE problems are considered to be important now by the majority of persons interviewed. These included problems #1, 3, 8, 9, 11 and 13 (Table 7.1).

Major activities and developments which contribute to CE problems include the James Bay hydroelectric project, mining, forestry, and industrial and municipal pollution. There are 56,460 ha of land in Quebec that have been disturbed by mining. Most mines are located in the southern half of the province. Elements being mined include iron, tin, molybdenum, nickel, gold, silver, lead, and zinc. In addition to these mining activities which discharge toxic waste, municipal waste discharges have resulted in major outbreaks of the alga Gonyaulax excavata and subsequent paralytic shellfish poisoning in the St. Lawrence River.

The forests in southern Quebec have been severely defoliated by spruce budworm outbreaks. In 1982, 1.2 million ha of forests were sprayed with insecticides. Only one percent of this area was sprayed with bacterial insecticide. The remainder (99%) was sprayed with chemicals. All of the aerial spraying occurred in the lower St. Lawrence River area and the Gaspé. The total forested area which has had trees killed by the spruce budworm in Quebec is 11,190,000 ha (Sterner and Davidson, 1983).

Ontario

CE problems which are of concern in this province relate to activities and developments associated with urbanization and industrialization in the south as well as mining and forestry in the north. In particular, the Great Lakes have been subjected to numerous studies to assess the impact from a number of mainly industrial pollutant sources on both the U.S. and Canadian side of the lakes. At present, the International Joint Commission is conducting a large multidisciplinary effort on fluctuating water levels in the Great Lakes.

Mining activities in heavily populated southern Ontario include non-toxic elements such as gypsum, salt, magnesium, and calcium. In central and northern Ontario, however, there are a number of metal mines (for example, iron, gold, copper, lead, tin, cadmium, and zinc). All of these mining activities have created substantial tailings areas where long-term rehabilitation is needed. In addition, the nickel ore smelter in Sudbury has been identified as a major contributor to the long-range transport of air pollutants. Emissions from this source together with the emissions from coal-fired electric generation stations constitute a major portion of Canada's contribution to the acid deposition problem of eastern North America.

Hazardous waste generation and its safe disposal are a particular concern in Ontario since this province generates over half of the hazardous wastes in Canada (Environment Canada, 1986). Of the 220 waste disposal sites in Ontario, approximately 50% are active. These sites are not considered to present a danger to health or to the environment at this time. Another concern in Ontario is the safe transportation of toxic and/or hazardous wastes by rail. There have been a number of toxic chemical spills in southern Ontario in recent years. The province has established regulations, and its Environment Protection Act tracks hazardous wastes from generation through to final disposal. In addition, the province has established the Ontario Waste

Management Corporation (OWMC) for the specific purpose of establishing a center with a comprehensive modern treatment technology. The selected site and technological proposals are now the subjects of a major environmental assessment submission under the province's Environmental Assessment Act.

Parts of Ontario are heavily populated, and the results of urbanization are now considered to be a major CEA issue. In particular, expansion of freeways, airports, and residential and industrial areas is considered to be rapid and extensive. Location of the rail system which conveys hazardous or dangerous goods in the vicinity of Metro Toronto is now the subject of a special task force. In addition, the safe disposal of solid wastes has become a major concern.

Prairie Provinces

The Prairie Provinces consist primarily of three ecozones: the boreal plain, boreal shield and the prairies. The types of cumulative effects which are considered to be important in these three provinces can, for the most part, be based on these ecozones. For example, the prairies are experiencing CE problems as a result of intensive agricultural practices which have changed the characteristics of the landscape. Drainage of wetlands, in particular, is a complex and serious problem, the ramifications of which have not been fully realized. Other problems such as reduced soil quality from compaction, erosion, improper fertilizer and chemical insecticide use, and salinization are also linked to the use of the area for agriculture.

The prairie ecozone is not extensively populated, but this is the most populated area of these provinces. The few major urban centers do not contribute substantially to cumulative effects problems in this area. Farming practices, however, have dramatically altered the landscape both physically and in terms of wildlife habitat.

Forests in the western foothills of Alberta (the boreal plain) have been subjected to outbreaks of disease, insects and poor harvesting practices. There has, however, been limited chemical spraying in this area. Insects and disease are largely controlled through the encouragement of selective cutting and burning. Oil and gas exploration have also had an impact on the biophysical environment in the Prairie Provinces through the construction of pipelines, seismic activity, tar sands development, and drilling activity.

The northern areas of the Prairie Provinces (including the boreal shield) are the location of a number of major projects and activities related to mining, oil and gas exploration and hydroelectric power projects. In northern Manitoba, for example, the Churchill River is the site of a major hydroelectric project that has resulted in a number of environmental impacts on the fisheries in the reservoir. In addition to an increase in mercury concentrations in fish species (and thus a decline in the economic value of the stocks and an increase in the health risk associated with eating the fish), there has been a change in the distribution and abundance of fish. The latter has resulted in exploitation of lower quality fish stocks. There are similar impacts associated with the hydroelectric power project on the Peace River extending into Alberta.

British Columbia and the Territories

Like the Prairie Provinces, British Columbia and the Territories have few major centers of urbanization. In British Columbia, agriculture and urbanization are concentrated in valley bottoms in the southern portion of the province with the natural resource harvesting (forestry and fisheries) occurring along the coast and in the northern portion of the province. Forestry, particularly, has been an increasing concern in terms of habitat fragmentation and alienation. Associated with the destruction of virgin forests and unique habitats are the problems of erosion on steep slopes and loss of biological diversity and stability. There are also a number of hydroelectric power projects in the province which affect the quality of freshwater fisheries.

The Northwest Territories and the Yukon, in particular, have few CE problems which are considered critical at this time. Although there are major problems that can be associated with oil and gas exploration, and mining developments, these problems are localized. One potentially serious problem in the Northwest Territories and the Yukon, however, is the cumulative effects associated with climatic modification. This could drastically alter the Arctic ice regime, the extent of permafrost, and the mean annual air and water temperature. The latter has the potential to alter distribution and abundance of both marine mammals and fish, and terrestrial mammals and other vertebrates.

Although there are no major projects affecting the hydrological regimes in the North at this time, there is the possibility that the government of British Columbia will dam one of the key tributaries of the MacKenzie watershed for hydroelectric power production. This would probably have serious repercussions for waterfowl, fisheries, and the characteristics of wetlands along the MacKenzie River. Most of the problems associated with the North, such as mining activities, and oil and gas development, are localized and affect only a small proportion of the total area of the Territories.

In summary, environmental problems that have already resulted or could result, in cumulative effects are evident in most areas of Canada in varying degrees of intensity. The heavily populated areas have problems related to urbanization, manufacturing, and industrial activities. The Prairie and Maritime Provinces suffer from poor agricultural practices and extensive forestry. Northern developments and activities such as dams for hydroelectric power may be isolated but could have far-reaching effects on both the aquatic and terrestrial environments. As discussed earlier, CE problems are usually interconnected both spatially and temporally. Problems which occur on land have the potential to affect groundwater, surface water and marine water (estuaries and coastal areas). CE problems are complex. Often the effects of activities are noticed before the activities have been identified as creating environmental problems. The potential CE problems that have been identified in this section for each province and territory do not constitute a complete list of potential or existing environmental problems but give an indication of the seriousness and potential extent of cumulative effects in Canada.

7.1.3 Ecological Interrelationships of Cumulative Effects

Of the four basic kinds of environments (atmospheric, terrestrial, freshwater, and marine) and six subcategories of aquatic ecosystems (groundwater, surface water, wetlands, estuaries, coastal areas, and open ocean) almost all are associated with the 16 identified CE problems. As shown in Table 7.2, air (or atmosphere) is the least affected by these problems but airsheds are affected by some of the most serious impacts related to cumulative effects, such as atmospheric fallout from nuclear testing or accidents, long-range transport of air pollutants (LRTAP) resulting in ecosystem acidification, and urban air quality. Climatic modification resulting in increased temperatures in the atmosphere, in particular, has the potential for several long-term repercussions.

The terrestrial ecosystem has the potential to be affected by all CE problems with the exception of #11 which refers specifically to aquatic ecosystems. Of the freshwater components, groundwater is impacted by land developments which contribute to chemical contamination or reduction in groundwater supplies as a result of activities such as the draining of wetlands or other recharge areas; the application of toxic chemicals in the form of fertilizers, insecticides, and herbicides; or toxic discharges from industrial operations. Surface water systems and wetlands, like terrestrial ecosystems, have the potential to be affected by all types of identified CE problems. Atmospheric fallout, terrestrial runoff, and groundwater seepage of toxins can influence the quality and quantity of lakes, streams and wetlands.

Estuaries and coastal areas such as bays and inlets are susceptible to CE problems originating from air and land sources. Discharges from municipal sewage systems have the potential to create problems in terms of biological contamination of shellfish species and loss of recreation areas. Major population centers tend to be located near or on estuaries; thus, these centers provide a number of pollutant sources. As these centers expand, the number, amount, and types of pollutants being discharged into the estuary grow exponentially.

7.2 Some Canadian Case Studies

The purpose of this section is not to catalogue case studies involving cumulative effects exhaustively; rather, by reviewing a few well-documented cases, to illustrate some of the methodological and jurisdictional issues associated with assessing effects of activities that result in cumulative changes to the environment. Overall, there are considerably fewer well-documented Canadian experiences in cumulative effects assessment than for traditional environmental impact assessment. In this section, particular attention is given to situations or issues already described by others.

7.2.1 Case Studies

The four Canadian case studies that follow are abridged versions of the originals presented by Sonntag et al. (1987) and Peterson et al. (1987).

Table 7.2 Ecosystems Affected by Cumulative Affects Problems (*)

CE Problems	ECOSYSTEM AFFECTED							
	Freshwater					Marine		
	Air	Terrestrial	Groundwater	Surface Water	Wetlands	Estuaries & Coastal	Open Ocean	
1. Long-range transport of air pollutants and ecosystem acidification	*	*		*	*			
2. Urban/rural air quality & airshed saturation	*	*		*	?			
3. Mobilization of persistent or bioaccumulated substances		*	*	*	*	*	*	
4. Climatic modification	*	*		?	?	*	*	
5. Occupation of land or water by man-made features		*	*	*	*	*	*	
6. Habitat alienation		*		*	*	*	*	
7. Habitat fragmentation		*		*	*	*	*	
8a. Decreases in soil quality		*	*	*	*			
8b. Decreases in soil quantity		*	*	*	*	*		
9. Effects of use of agricultural, silvicultural, and horticultural chemicals		*	*	*	*	*		
10a. Reduction of groundwater supplies		*	*	*	*			
10b. Groundwater contamination		*	*	*	*			
11. Increased sediment, chemical & thermal loading of freshwater and marine habitats.				*	*	*		
12. Accelerating rates of renewable resource harvesting		*		*	*	*	*	
13. Long-term containment & disposal of toxic wastes		*	*	*	*	*	*	
14. Activities and developments producing carcinogenic-teratogenic effects	*	*	*	*	*	*	*	
15. Loss of biological diversity		*		*	*	*	*	
16. Change in hydrological regimes of major rivers/estuaries.		*		*	*	*		
17. Loss in ecosystem stability		*		*	*	*	*	

Additional insights were derived from CEARC (1988) written after the other reports were submitted. The case studies are:

- Fraser River Estuary;
- New Brunswick Forest Management;
- Land-use Practices, Habitat Fragmentation, and Soil Changes in the Prairie Provinces; and
- Leaded Gasoline.

Some major methodological and jurisdictional issues raised by these cases are summarized at the end of the section.

Fraser River Estuary

This case study, reported by Sonntag et al. (1987), is useful for highlighting the role of regional planning in dealing with cumulative effects issues. The emphasis, therefore, is largely on institutional aspects of CEA.

The estuary of the Fraser River in British Columbia is part of a major ecological system, dominated by the Fraser River, that now includes one of Canada's major metropolitan areas. The Fraser River is renowned for its salmon runs, while it also supports the largest population of wintering waterfowl in Canada and is an important stopping point on the Pacific flyway for migrating birds. Human settlement has radically changed the lower Fraser Valley over the last fifty years: dyking channels the river; two-thirds of the original wetlands have been drained; and forests on the valley floor have been replaced by agriculture and urban development. Increasingly, commercial fishing fleets are sharing moorage with international trade and recreational boats, while industrial effluents, sewage, and run-off are discharged throughout the estuary. Such a scenario is almost guaranteed to result in a variety of cumulative effects, issues, and concerns.

From the Fraser case study, it becomes clear that much is known about the cumulative degradation or loss of ecosystems in the Fraser estuary and the factors contributing to this degradation.

Sonntag et al. (1987) discussed the institutional role that regional planning has played in initiating programs designed to control activities that result in cumulative effects, and they traced the evolution of regional planning programs and authorities in the lower Fraser River area. The authors also discussed how EIA helped develop the information base.

One specific example of how impact assessments have helped address cumulative effects in the Fraser estuary involves the proposal in the early 1970's to expand Vancouver International Airport onto Sturgeon Banks. This stimulated public concern for the cumulative consequences of developments in the estuary. As one of the first projects submitted to the federal Environmental Assessment and Review Process Office (EARP), the proposal led to intensive questioning of both the biophysical and socio-economic consequences of the project. It also stimulated similar questions about other developments in the estuary. These concerns resulted in several activities by government authorities. In 1977, the provincial Cabinet approved Order-in-Council 908

(requiring environmental impact assessments for developments outside the dykes), and the federal and provincial governments signed an agreement to undertake the Fraser River Estuary Study (FRES).

The FRES program, still continuing, was initiated to define the estuary management problem, to formulate management strategies, and to evaluate alternative institutional arrangements for ongoing management. While there have been notable successes in this program, it is also clear that major challenges await solutions. For instance, developing institutional responses is still a lengthy process, and ongoing political controversy and lack of political commitment to regional planning make the job more difficult just at a time when cumulative effects issues are becoming more numerous and difficult to predict. Success in dealing with these issues will, according to Sonntag *et al.* (1987), depend on continuing to learn how to integrate planning and impact assessment techniques and processes to provide a timely basis for action.

New Brunswick Forest Management

This case study, also reported in Sonntag *et al.* (1987), provides a good example of how poor environmental problem definition can delay the process of seeking and implementing a solution. New Brunswick has the largest proportion of forested land of any province in Canada. Economic development has traditionally involved exploitation of the forest resource to promote local and regional economic growth. Simultaneously, however, undesirable local ecological impacts from harvesting have resulted in regional degradation of the New Brunswick's forest industry.

Although many of the factors contributing to these cumulative effects were in operation since at least the 1950's, resource degradation was not recognized as a significant problem until the early 1970s when local industry began to evaluate why they were losing competitiveness in world trade. Simply stated, the problem resulted from the accumulation of consequences of many small-scale interventions by man and natural agents (for example, harvesting and budworm infestations) that occurred over approximately seventy years. The result was not only a product of poor quality (unusable species), but also a projected shortfall in the volume of raw material. Once it was agreed that maintaining the flow of quality material was the real problem, emphasis on designing and implementing long-term corrective measures soon followed (Regier and Baskerville, 1985).

From the description in Sonntag *et al.* (1987) it is unclear what ecological methods were used to analyze the cumulative effects problem in the New Brunswick forests. The authors, however, provided insights into the institutional mechanisms that were used to deal with the issues, once they were identified.

The projected shortfall in timber supply led the province to commission a forest resources study in 1974 (Province of New Brunswick, 1974). The resulting report advocated establishing a new forest management program and producing comprehensive guidelines for forest resource development. Moreover, new legislation was initiated to bring control of timber licenses under one

agency, the Department of Natural Resources. In addition, in 1980, the province passed the Crown Lands and Forests Act to reallocate access to Crown timber (Province of New Brunswick, 1980). This Act has now made it possible for the provincial government to rigorously design and control forest management. The Act also provides incentives for silviculture to improve the wood supply situation. This case study shows that even in a province that demonstrated the ability to react positively and rapidly to a cumulative effects problem, there was still a lag of ten years in applying methods for the purpose of identifying and quantifying the nature and significance of the effects.

Changes in Land-use, Habitat, and Soils in the Prairies

This case study, as described by Peterson *et al.* (1987), focused on habitat fragmentation and soil changes that result from land use practices determined mainly by thousands of individual land owners. Of all the case studies, this is perhaps the best illustration of the "tyranny of small decisions". This case study has been treated as a CEA feasibility study in Volume II of this Guide (Wallace and Lane, 1988). The area of concern is the zone extending from the Peace River region of northeastern British Columbia across the northern fringe of the prairies to southeastern Manitoba. Along with agricultural areas, this zone also includes the forestry-agriculture interface along its northern and western edges. In this region most ecosystem disturbances are from less intensive land uses. Notable exceptions include urban areas, mining for oil sands and coal, and reservoirs.

The dominant institutional and socio-economic feature of this region is the exceptionally large number of individual farms, many of which are still family operations. Thus, the decision making process controlling land use practices and, hence, habitat fragmentation and soil changes, is highly decentralized. To assess effects of land use practices and agricultural chemical use on any given site is difficult; to evaluate the effects of these practices on a regional basis is even more difficult.

Peterson *et al.* (1987) discussed the fact that given these circumstances, standard EIA methods are inadequate. They discussed ways in which the problem could possibly be managed, and they suggested that economic incentives, educational and extension programs, and perhaps planning procedures might all be appropriate. Since few of the cumulative effects associated with agricultural practices are perceived as representing a serious or immediate threat to human health, neither public concern nor political will exists to force changes. Furthermore, many of the cumulative changes are difficult to detect. In the absence of data to document these changes, it is difficult for many of the decision makers to realize the need to change current land use practices.

Despite the fact that a wide range of provincial and federal agencies have interests and mandates to help in managing the land and soil resources, there seem to be few attempts to coordinate efforts. Furthermore, examples exist of programs administered by one agency actually helping to cause the effects that another is trying to resolve. Together, all the factors described above help prevent the discovery and application of solutions. Until leadership is

forthcoming, it is likely that land use practices will continue to be guided more by economic forces than by the need to conserve land and soil resources.

Leaded Gasoline

In this case study by Peterson et al. (1987), cumulative effects associated with the use of leaded gasoline are examined in light of how the effects were first assessed, and then used in drafting legislation to control use of this product. Leaded gasoline use produces airborne emissions of particulate lead. Not only do these emissions result in elevated lead levels in the atmosphere, but lead concentration levels also increase in water, urban topsoil and dust, and in foods. Since lead is persistent in the environment and accumulates in animal tissues and water and plant cells, the possibility for cumulative effects is obvious. Because of continual exposure directly and through the food web, human lead levels are rising correspondingly.

As Peterson et al. (1987) pointed out, the regulatory actions in this case were initiated only after overwhelming scientific evidence had been collected demonstrating that cumulative effects had already occurred. This delay seems to have been related to the fact that even though scientific evidence linking cumulative doses with human responses was available, economic data played a much larger role in influencing legislation to control leaded fuels.

After studying this case in detail, Peterson et al. (1987) suggested that even though scientific methods may be applied for analyzing a cumulative effects problem, the results will not necessarily be used in efforts to solve the problem. Thus, an important point when evaluating the "effectiveness" of any method designed for assessing cumulative effects is whether the results will be understood, and used, by decision makers.

To deal effectively with problems such as leaded gasoline, all related environmental consequences of an activity will have to be assessed in an integrated, holistic environmental assessment. It seems likely that had such an assessment been done for lead in the 1920's, when anti-knock additives were first proposed, human health concerns would have indicated the unacceptability of discharging this substance into the atmosphere.

7.2.2 Some Conclusions

The four case studies outlined above offer insights into both the methodological and jurisdictional concerns associated with cumulative effects problems; these are summarized in the following paragraphs.

Recently, considerable attention has been devoted to the question of what methods and procedures are most appropriate for use in cumulative effects assessments. As the leaded gasoline case study illustrates above, there are reasons to suspect that the availability of appropriate methods or procedures is not the bottleneck preventing CEAs from being undertaken. Nevertheless, more advanced methods are required for use in CEA than in traditional EIA. For example, in cases where cumulative effects are associated with thresholds, methods for identifying and quantifying these thresholds are required. Likewise, difficulties in quantifying the rate and extent of soil degradation

and loss are related, in part, to the lack of methods for carrying out such analyses. The paucity of methods for evaluating the significance of losing part of an estuary, or a forest, or any ecosystem is likely to at least cause difficulty when attempting to assess cumulative effects.

Even if methods, such as those called for above, were available, they would not only have to be used in CEA, but also interpreted by scientists and decision makers. This calls into question the issue of whether the overall framework that has been developed to date is adequate. The question of how to interpret results from CEA is not trivial. If methods used to measure cumulative effects are complex, then the results may be difficult to convey to decision makers. In such cases, it may be difficult to judge whether or not the CEA was worthwhile.

A frequently asked question surrounding cumulative effects issues is: "Who should accept responsibility for carrying out the assessment?" In the case of a typical development project requiring an EIA, it is generally accepted that the proponent will be held responsible for having the assessment prepared. It is much less clear who should carry out the assessment of cumulative effects of, for example, agricultural practices in the prairies or logging in New Brunswick.

Presumably, most, if not all, of our current government and social institutions were created to deal with problems that existed at the time of their creation. As new issues and problems emerged, mandates increasingly began to overlap. While admittedly oversimplistic, this helps explain some of the confusion over who is responsible for dealing with cumulative effects issues. In most cases no one existing institution is responsible for dealing with this issue, and it is often more complex than any one contemporary institution appears able to manage effectively. This is not an insoluble problem but its solution will require some changes in Canada's present assessment process or more likely, in institutional systems.

Since jurisdiction also implies responsibility, and responsibility is at the heart of all cumulative effects problems, who, for instance, is willing and able to take responsibility for evaluating cumulative assaults on the environment? Equally important, who will decide when a threshold has been reached or surpassed, and what to do about it?

Few of the issues that are currently regarded as examples of cumulative effects are truly new. Most have been known for some time under other names such as land use, habitat fragmentation, and soil degradation issues discussed for the prairies. Nearly all of these problems were well known in the 1930s. What prevented action then, as now, was not primarily a lack of understanding, but rather a lack of coordinated efforts to develop and implement solutions. Redesigning some institutions to remove jurisdictional barriers will be an important step toward promoting cooperation and joint problem-solving.

7.3 U.S. Experience In CEA

Since the late 1970's, the U.S. Council on Environmental Quality has included cumulative effects in regulatory policy. The National Environmental Policy Act (NEPA) is usually acknowledged for requiring cumulative impact (CI)

assessments even though NEPA does not specifically mention cumulative impact (CI). (Note that cumulative impacts [CI] and effects [CE] are used interchangeably in this Guide although U.S. custom tends toward CI and Canadian custom toward CE). The Council of Environmental Quality has subsequently promulgated regulations that require consideration of CI for implementing the NEPA. In the last ten years, there has been an intensive effort in the U.S. to develop CEA methodologies which have often been applied in a case study format. Several state and federal agencies have been involved in these efforts as well as a number of major proponents. In particular, the U.S. Fish and Wildlife Service at Fort Collins, Colorado, has had a cumulative impacts research team in place for several years. It has been specifically charged with developing a cumulative effects process. In total, the CEA studies in the U.S. have been diverse in terms of philosophy, approach, type of ecosystem and perturbations, methodologies, format, conclusions and recommendations. These very disparate efforts are difficult to summarize succinctly. It is important for the reader, however, to be aware of this diversity and have a general guide to which groups are using which methodologies. With better liaison between U.S. and Canadian assessment efforts, more progress could be made on CEA in both countries. It is clear that careful review of U.S. efforts will provide cost and time savings here in Canada, and we should plan our CEA efforts with enhanced U.S.-Canadian liaison. Several references describe U.S. approaches to CEA and provide a useful context for the methodological techniques listed therein (Cline et al., 1983; Horak et al., 1983a,b).

7.3.1 Current Federal Agency Involvement in CEA

The current status of major federal agency work on cumulative effects in the U.S. is briefly summarized in Table 7.3. Each federal agency and its main CI activities are briefly described below. Individual states and private proponents are also taking an active interest in CEA, but these diverse efforts are difficult to summarize in a short space.

Bonneville Power Administration, USDE

Concern over cumulative effects resulting from hydroelectric projects in the Pacific Northwest has been increasing in recent years. The Northwest Power Planning Council adopted cumulative effects provisions in the Columbia River Basin Fish and Wildlife Program in 1982. In support of those provisions, Bonneville Power Administration (BPA) funded Argonne National Laboratory (ANL) to develop criteria and a method for assessing potential cumulative effects of hydroelectric developments on fish and wildlife (Bain, 1985,b,c). BPA also funded a study to develop and apply methods to evaluate the cumulative effects of twenty proposed small hydro projects on fisheries resources in the Swan River drainage of Montana. Economic as well as fisheries impacts were included in that study (Leathe, 1985).

Army Corps of Engineers, USDOD

The Army Corps of Engineers (ACE) developed procedures for analyzing cumulative impacts at a relatively early date (Dames and Moore, Inc., 1981). This resulted in a comprehensive and detailed handbook that reviewed the legal

Table 7.3 Involvement of U.S. Federal Agencies in Cumulative Impacts (CIs) Assessment

Agency	Major Methods	Case Study or Process	Principle References
Bonneville Power Admin. USDE	Habitat Analysis, Fish Populations, Economics, Hydrology	Swan River Drainage Montana Hydro- electric Development	Leathe <u>et al.</u> , 1985
Army Corps Engineers USDOD	General CI Analysis Including Public Interest Review	Draft Handbook of CI methodology	Dames & Moore, Inc., 1981
Environmental Protection Agency	1) Island Biogeo- graphy AEAM 2) Collaborative Ecological Risk Assessment	1) Bottomland Hardwoods 2) Process Development 3) Freshwater Wetlands	1) Gosselink & Lee, 1986 2) Preston & Bedford, 1887
Federal Energy Regulatory Comm USDE USDA	Cluster Impact Assessment Process	Salmon River and Snohomish River Hydroelectric Development	1) Bain <u>et al.</u> , 1985a,b,c 2) U.S. FERC EIS., 1987
Forest Service USFS	General Process Framework	General Guidelines	1) USDA, 1981 2) USDA, 1984a,b 3) Salwasser & Samson, 1985
National Park Service USDI	1) CI Problem Solving 2) Least Acceptable Change	1) Glacier National Park 2) Denali & Other Parks 3) Alaska National Park	Raley <u>et al.</u> , 1987
Fish and Wildlife Serv. USDI	General CI Problem Solving Process	1) Chesapeake Bay 2) Mobile Bay 3) Great Lakes, UMR 4) Upper Mississippi	1) Williamson <u>et al.</u> , 1986 2) Armour <u>et al.</u> , 1984

basis for cumulative impact (CI) assessments and describes CI assessment procedures needed to meet CE jurisdictional mandates. This includes physical, chemical and biological changes as well as economic, social and behavioural effects, because the ACE has responsibilities for balancing differences in public interest reviews. Both positive and negative impacts are discussed, as are direct, secondary, indirect, future, aggregating and growth-inducing effects.

ACE has not officially adopted the approaches outlined in the Dames and Moore Draft Handbook, and the Handbook is not generally available. Other documents by ACE (for instance, INTASA Inc., 1981a,b and Horak and Vlachos, 1984) have incorporated many of the recommendations made by Dames and Moore, Inc. The ACE has explicitly addressed CI as part of their extensive regulations for controlling certain activities in waters of the United States.

Environmental Protection Agency

The Environmental Protection Agency (EPA), in cooperation with the FWS, completed three workshops concerning ecological impacts in bottomland hardwoods. Cumulative impact was not the intended purpose or direction of these workshops, but CI analysis became an important part of understanding the ecological implications of diverse activities occurring throughout large watersheds or river basins (Roelle *et al.*, 1985; 1987). One subgroup argued that there are important levels of analysis (watershed, regional landscape) above the site-specific (local) ecosystem and, furthermore, that emerging scientific understanding of processes is sufficient to formulate regulations at these levels under the general heading of cumulative impacts. A basic approach was briefly outlined for several higher-level functions of bottomland hardwoods, such as maintenance of natural biotic diversity. A general landscape ecology approach to CI in bottomland hardwoods was also described as a result of this project (Gosselink and Lee, 1987).

EPA has a project connected with its Corvallis Environmental Laboratory to develop a sound scientific basis for investigation of cumulative effects on freshwater wetlands. A workshop was held in 1986 involving presentation of papers regarding cumulative effects on wetlands, and the proceedings are forthcoming. EPA is also working with the FWS to develop ecological risk assessment procedures that can be used to estimate biological thresholds. Two workshops have been conducted in this project, and reports are in preparation.

Federal Energy Regulatory Commission, USDE

The Federal Energy Regulatory Commission (FERC) has no specific regulations on cumulative impacts. The FERC can issue preliminary permits, licenses and exemptions on a case-by-case basis with no review of a particular project application in relation to other proposed projects in the same river basin. The Commission has stated, however, that it would honour the recommendations of relevant agencies when certain project impacts cannot be mitigated and residual impacts remain. Some of these involve cumulative effects.

In April, 1985, the FERC developed the cluster impact assessment procedure (CIAP) to evaluate the cumulative effects of multiple hydroelectric projects

in river basins. The CIAP was applied to three pilot river basins, including two listed in the next section. The CIAP geographic areas of concern that could have adverse effects on target resources have been identified. FERC's current position is that CI studies are only needed when licensed projects are clustered in a river basin, not when they are dispersed.

Forest Service, USDA

The Forest Service (USFS) issued regulations supplementing NEPA-implementing procedures in 1981 stating that impacts may be direct, indirect or cumulative (USDA, 1981). In 1984, the USFS published a notice of proposed revisions of NEPA-implementing decisions, including a more distinct definition of CI (USDA, 1984a). Teams conducting environmental analysis must have the professional capability to identify and evaluate the potential direct, indirect and cumulative, social, economic, physical and biological effects of proposed actions and alternatives.

Guidelines for implementing cumulative effects analyses were provided in an unpublished report in 1984 (USDA, 1984b). The purpose of the report was to provide a framework of assumptions and principles that should be considered during CI analysis in forest planning. The process is generic, with criteria to be developed and applied based on individual situations. The cumulative effects task force further elaborated a generalized process for use in forest planning for the American Forestry Society (AFS) (USDA, 1987; Salwasser and Samson, 1985). Process steps include CI description, spatial bounding, threshold descriptions, data collection, effect prediction, mitigation determination, and monitoring.

National Park Service, USDI

The National Park Service (NPS) develops master plans for each of the units under its jurisdiction. Unlike the Fish and Wildlife Service, NPS has strong jurisdictional authority over the natural resources it is responsible for protecting. CI studies on Glacier National Park in 1986 and Denali National Park in 1987 grew out of management planning activities. The NPS is currently developing a CI process based on limits to acceptable change (LAC) for trial use in Denali and other Alaska park units. These studies are briefly discussed in the next section.

U.S. Fish and Wildlife Service, USDI

The U.S. Fish and Wildlife Service (FWS) in 1982 funded a project entitled, "Methods for Determining Cumulative Effects of Coal Activities on Fish and Wildlife Resources". The overall goal of the study was to identify and summarize the state-of-the-art of biological assessment and monitor the cumulative effects of development on fish and wildlife populations and habitats. Three documents were developed and first reviewed by an interdisciplinary working group and later by attendees at a national workshop and a technical review session, both held in July, 1982.

The first document, "Fish and Wildlife and Cumulative Effects: Is There a Problem?" (Horak et al., 1983a), was developed primarily for policymakers and

the public. It emphasized a non-technical discussion of the general cumulative impacts issue. A classification system was proposed to represent the broad range of cumulative impacts. Fish and wildlife may be affected by many projects of the same type, individual projects involving different types of activity, or the combined effects of two or more actions on various wildlife habitats and species.

The assessment of project impacts is viewed as part of a larger process for tracing the effects of change on biological, physical and social environments. Recommendations to improve CI assessments therefore emphasize the importance of cooperation among agencies and industries. Government agencies must become more familiar with the motivations and actions that industries follow in pursuit of their economic development objectives. In general, decision makers need to understand the goals, mandates and methods of other organizations if CIs are to be managed more effectively. It is further recommended that approaches which allow for a range of possible outcomes leading to decision making be examined through case studies, workshops or national conferences. Field biologists need training in the broader institutional setting of CI issues including terms, characteristics and methods for assessing cumulative impacts. Basic research on specific ecological functions and processes, and alternative institutional mechanisms was also recommended.

The second document, "Methodological Guidance for Assessing Cumulative Impacts on Fish and Wildlife" (Horak et al., 1983b), provided interim guidance for field biologists who must assess cumulative impacts. It focused on methods then available with potential utility in CI assessment.

A cumulative impacts research and development project was initiated in 1984 at the National Ecology Center (NEC) which is part of the USFWS in Fort Collins. The CI project was designed to follow some of the important recommendations made in the Horak et al. (1983a,b) work. In particular, a collaborative problem solving (CPS) approach is being used to develop cause and effect analyses to narrow the problem and establish causal pathways for more intensive research and management decisions. Collaboration vertically within organizations, among relevant disciplines and among institutions is stressed throughout the CI project. This can best be accomplished through interactive workshops conducted periodically through the design, development and implementation phases of the project.

The emerging FWS/NEC CI process is based on general problem solving approaches such as those described by the National Research Council Committee on the Application of Ecological Theory to Environmental Problems (1986). Familiar steps of problem definition, analysis, consideration of alternatives, and monitoring are used by project-specific work groups to organize a CI assessment. Flexibility in choosing specific approaches is essential because of highly variable legal, institutional and budgetary constraints. The approach is summarized in Table 7.4.

7.3.2 Case Studies

Table 7.5 lists case studies and projects which the cumulative impact project team of the U.S. Fish and Wildlife Service/National Ecology Center

Table 7.4 Collaborative Problem Solving Process (CPS)
(R. Johnson, pers. comm., FWS, Fort Collins, CO)

STEPS

- 1) Identify the Problem:
 - o Conduct CPS workshops to:
 - identify the problem
 - establish preliminary cause and effects
 - bound time, space and concepts
 - match legal institutional and regulatory entities (LIR)
- 2) Develop Conceptual Hypothesis:
 - o Determine CI classification
 - o Determine methods or models
 - o Conduct CPS workshop to:
 - finalize cause/effect understanding
 - design empirical analysis
 - assign work
- 3) Analyze Problem (test hypothesis):
 - o Review literature and collect data
 - resource inventory, status and trends
 - LIR and socioeconomic considerations
 - o Build and execute models and methods
- 4) State Results and Conclusions:
 - o State results of empirical analysis
 - o Relate and predict consequences for:
 - environmental management objective
 - LIR and socioeconomic policy implications
- 5) Make Recommendations and Process Revisions:
 - o Evaluate study and results
 - o Recommend use of study results
 - o Recommend future work
 - technical methods
 - LIR and socio-economic considerations
 - o Design monitoring and feedback evaluation system.

participated in or observed. It should only be viewed as representative of U.S. work in CEA, not an exhaustive list. We briefly elaborate on these studies in this section.

FWS/NEC CI Project

A collaborative problem solving (CPS) process is being used to develop and implement a cumulative impact assessment process for FWS/NEC. A brief description of recent FWS involvement in CI assessment, and the description of the CI process were provided in the previous section. We list that project again here in order to illustrate the breadth of applications addressed by the CI process, and to describe how various methods were used. Whereas in the previous section we described a specific CI assessment, in this one we discuss how the CI process is being developed.

Initially, traditional literature review procedures were employed. The FWS documents produced by Horack et al. (1983a,b) were the most useful references. Some conceptual underpinnings achieved in that study allowed for a temporary by-pass around definitions and typologies to provide recommendations for future work. An adaptive and cooperative approach utilizing more than one technique, heavily conditioned by institutional realities, was incorporated into the design of the FWS/NEC CI project. The Horack et al. (1983a,b) studies also provided justification for involving representatives of several organizational levels within the FWS, various scientific disciplines and diverse institutional interests in designing the project. The NEC director (then Leader of the Western Energy and Land Use Team, which NEC replaced), the Director of the Division of Biological Services, and the Director of Ecological Services were active in technical as well as management and administrative decisions all within FWS. All three of these leaders participated in a three-day CI project scoping meeting and attended a field supervisor's conference where CI was selected for discussion as a high priority issue in the FWS. This support by high level officials provided managerial expertise and policy relevant to the study design, contributed technical expertise, and legitimized the enthusiastic participation of others in the project.

Field biologists, field office supervisors and relevant regional office representatives also participated in these early meetings. Their participation contributed essential technical and pragmatic institutional expertise, began a training function and enhanced implementation opportunities through case studies. The general CI project was designed during these early meetings (Williamson et al., 1985).

Several methods used during problem identification and scoping for the CI project were later used in case studies and will be included in the menu of methods for various tasks in the general CI process. In particular, nominal group techniques (Delbecq et al., 1975) and cause/effect analysis (Armour et al., 1985; Williamson et al., 1986) have become valuable tools in cumulative problem solving-process (CPS).

Analysis in the CPS approach is dominated by the use of a case studies. These case studies may be viewed as addressing the implicit hypothesis that a CPS

Table 7.5 Case Studies and Projects which the Cumulative Impact Project Team of the U.S. Fish and Wildlife Service/National Ecology Centre have Participated in or Observed.

Case Studies and Projects	Type and Purpose	Institutional Jurisdiction	Scoping Method
FWS/NEC CI Project	-Process Development	-FWS -other agencies -science	-CI Process -cause/effect -nominal group technique
Ohio, Tennessee Cumberland Rivers	-Barge Traffic	-CI: -FWS, states, & other agencies	-Cause/effect
Bottomland Hardwoods	-Regulatory Policy -Process devel.	-EPA: CI, FWS, states, other agencies	-AEAM scoping meeting
Snohomish River	-Low-head hydros and -process devel.	-FERC FWS, NMFS, Tribes state & other	-FERC's CIAP
Salmon River	-Low-head hydro & process development	-FERC: FWS, NMFS, Tribes State & other	-FERC's CIAP
Chesapeake Bay	-Bay recovery plan	-EPA - FWS -private, state federal agencies	-CI process -cause/effect -nominal group technique
Mobile Bay	-Bay recovery	-CI: FWS -States, local	-cause/effect
Upper Mississippi River	-recreation plan -barge traffic	-CI: FWS -UMR Basin Comm. inc. state & fed. agencies	-collaborative workshops
Ecological Risk Project	-process devel. -thresholds and	-EPA: FWS -Universities	-interagency meetings and

Table 7.5 (cont'd.)

Case Studies and Projects	Type and Purpose	Institutional Jurisdiction	Scoping Method
Great Lakes	-international resource project and planning	-FWS: -GLS coord. comm. included	-CI process
Glacier Park	-park master plan	-NPS -FWS -other agencies	-interagency meetings & correspondence
North Slope Alaska	-energy devel. -regulation	-FWS: USDI -State of Alaska -Oil firms -Public	-interagency meetings -cause/effect
National Parks, Alaska	-placer mining -process devel.	-NPS: EPA -FWS, state of Alaska	-interagency meetings and correspondence

approach, utilizing real world learning experience, can most easily lead to an effective CI assessment process (Table 7.4). The case studies (listed in Tables 7.3 and 7.5) are briefly discussed as observations or experiments in support of that implied assumption.

Ohio Tennessee and Cumberland Rivers

The Ohio, Tennessee and Cumberland Rivers (OTCR) case study was a very short-term project for the FWS/NEC CI project (CIP) team. The FWS Field Office in Cookville, Tennessee, prepared comments on CI permits regarding barge traffic on OTCR. The CIP team spent a total of two man-weeks assisting the field offices in developing and presenting a cursory cause/effect (C/E) analysis (only about two days were spent in actual C/E modeling). Most of the coordination for the work was accomplished by telephone and by mailing documents. Two CIP Team members from NEC attended an inter-agency working meeting in Cookville to provide briefings on the C/E model and learn more about the CI problem (Figure 7.1).

Information available from the C/E model was sufficient to meet the needs of ACE and the FWS field office in determining principal resources of concern, relevant perturbations and causal pathways. Collaboration between two field offices of the FWS was enhanced by the CI assessment. The major contribution of this project to the development of a general CI process was the successful use of C/E analysis over the short time frame and modestly funded project. The C/E model did not provide quantitative answers to detailed questions, but it did provide a mechanism for consensus between agencies about important variables to work with, and a framework for tracking potential effects of barge traffic on fish and wildlife resources.

Bottomland Hardwoods

The Bottomland Hardwoods (BLH) case study was completed with NEC facilitated modeling workshops through an inter-agency agreement between the FWS and EPA. The initial thrust of the study was toward agency policy development, not to develop CI processes or to assess CI. One member of the FWS/NEC cumulative impacts project team, however, was involved in the project, and CI assessment became a major issue during the course of the study. A scoping meeting and three workshops were managed by a modified adaptive environmental assessment and management (AEAM) process (Holling *et al.*, 1978a,b). Simulation models were not constructed, but the general AEAM facilitated workshop approach was utilized to develop word models and data compilation regarding the functions and attributes of bottomland hardwoods and the likely environmental implications of alternative BLH development actions.

A subgroup, established to consider CIs and methods to assess them, concluded that incremental analysis of site-specific development activities could not be accomplished effectively with the existing institutional-jurisdictional framework. Moreover, mechanisms for establishing ecological objectives for spatial areas that reflect cumulative effects were not available. One recommendation growing out of this work was that landscape ecology and island biogeography be used to assess CI (Gosselink and Lee, 1987).

This case study exhibited good collaboration within relevant units of EPA and between resource enhancement agencies. Representatives of permitting agencies and resource development interests were included in the workshops. Economists were invited to one of the workshops in an attempt to seek interdisciplinary strength for evaluating, identifying, and describing, important functions identified in the Bottomland Hardwood Case Study (Gosselink and Lee, 1987). The most important contribution of the BLH study to CI process development was the determination that although ecological goals are essential, they frequently are not available at scales that match CI problems. The description of landscape ecology and how it might be used in CI assessment is an important advancement.

Snohomish River and Salmon River

The Federal Energy Regulatory Commission (FERC) conducted two separate cumulative impact assessments as part of the NEPA-required regulatory compliance for licensing small hydroelectric projects. These studies were located in the Snohomish River Basin in Washington and the Salmon River Basin of Idaho where multiple hydroelectric projects were proposed.

The scoping meetings for each project followed the scoping process outlined in FERC's clustered impact assessment procedure (CIAP), which consisted of several interactive meetings between the study participants and federal and state natural resource agencies to develop the study scope, gather information, and evaluate the assessment methodology. The actual assessment of impact was carried out using a variant of the Argonne multiple matrix methodology (Bain et al., 1985a,b,c), developed by Argonne National Laboratory (ANL) who participated in the study with FERC.

The matrix-based approach was designed to accumulate impacts from the assessment of single-project effects. Target resources, such as fish or wildlife species, were identified and for each target resource, a number of resource components were identified which would be important to the target resource. For the Salmon River and Snohomish River studies, the impact of each project on each target resource component was assessed in terms of a numerical criterion. Assessments for all projects for a single resource component formed a matrix vector. A project by project matrix was then formed, in which were placed elements expressing whether the impacts of each possible pair of projects could interact in a nonlinear way. This matrix was called the interaction matrix; and, when the interaction matrix was multiplied by the vector of project impacts the cumulative impact of all projects on a resource component was estimated. Adding the cumulative impacts for all resource components for a single target resource resulted in an assessment of the cumulative impact of all projects on the target resource. Ecosystem effects, such as the relationship between eagles and salmon were entered into the analysis at the resource component level, salmon being a resource component for eagles.

The results of the cumulative effects assessments were used in the decision making process after examining the cumulative effects of all possible combinations of projects on each target resource. Since cumulative effects assessment de-emphasizes the significant single-project effects, project

combinations in which highly significant single-project effects were present were flagged for special consideration. Project combinations which either were flagged or had high cumulative effects on any target resource were eliminated from consideration as a licensing scenario. The studies resulted in recommendations to the FERC commissioners for alternative licensing scenarios, and final EISs have been issued.

Collaboration of natural resource agencies in the cumulative assessment process was sought, but success was limited. The scoping and licensing process was generally unresponsive to agency concerns and review comments. Since new information regarding the projects could not be sought due to the time frame in which a decision was to be made, impacts were expressed in non-quantitative terms. This was unacceptable to several resource agencies. The use of evaluative criteria was very problematic and did not satisfy those agencies requesting impact quantification or those agencies opposed to impact quantification. Whereas FERC took a large and new step toward collaboration and the assessment of cumulative impacts, the urgency and visibility of the study, and the importance of the resource under study, made collaboration and testing of the methodology very difficult.

Chesapeake Bay

The Annapolis, Maryland, Field Office of the U.S. Fish and Wildlife Service (FWS) is conducting a CI assessment in connection with the Chesapeake Bay Restoration Plan. The general motivation for FWS involvement in this work was observed declines in waterfowl and fish populations in the Bay.

Many agencies are involved in this project. EPA has a primary role and has been emphasizing water quality in the Bay. The FWS is pursuing an emphasis on living resources with the FWS/NEC CI problem-solving process. Two collaborative workshops have been conducted to define problems, identify important cause/effect relationships and develop action plans. Submerged aquatic vegetation is one of the key variables being addressed as a consequence of the cause/effect analyses. Attempts to enter the CI issue from a general problem statement, such as declining or unacceptable water quality, proved unsatisfactory in this case study. Even as issues were narrowed from that point through cause/effect analysis, there was no clear progression toward species populations, habitats or geographical units that could be meaningfully investigated. When the Bay restoration issue was entered from a wildlife species and habitat approach, causes and consequences led more quickly to specific problems and remedial actions.

The FWS/NEC approach has not yet been accepted by a six agency organization working on the Bay Restoration Plan. This may be attributed to the lack of involvement of other agencies in designing the CI assessment. Other agencies and institutions have their own goals and mandates, and until a CI assessment fits those needs it will not likely be used. Agencies, including the FWS, are reluctant to adopt CI procedures because of uncertainties regarding precedent setting consequences of methods and decisions, lack of confidence in the methods used, and acceptability by Congress and other constituencies of the results of CI assessments.

Mobile Bay

Multiple impacts to wildlife have occurred over large temporal and spatial scales in Mobile Bay, Alabama. For example, a 40' deep and 400' wide navigation channel requires dredging maintenance now and may be expanded to 50' by 550' in the future. Part of the Bay would be filled with dredge material from the expansion project. Chemical contamination, and development of the Bay for industrial and urban uses also continue to contribute toward fish and wildlife habitat degradation and population reduction. The first objective of an FWS/NEC cumulative impact study in Mobile Bay was to improve the documentation of these continuing wildlife impacts through more careful trend analysis and CI assessment procedures. Strong institutional objectives and jurisdictions for protection of natural habitats are currently missing for Mobile Bay. A second objective therefore was to establish a forum where objectives for the Bay could be articulated and followed up. A research plan leading toward pragmatic decision making was also considered a requirement. Accomplishment of these two objectives could lead to an overall plan for resource enhancement for the Bay area.

A scoping meeting and a cause/effect workshop have been conducted by FWS/NEC. The cause/effect (C/E) model developed during the workshop emphasizes changes in Bay geometry, turbidity, contaminants and loss of submerged aquatic vegetation as key problem areas for fish and wildlife resources. Data to specify the C/E model more quantitatively are currently being developed. The objectives of subsequent workshops were to improve the C/E models and enhance interagency cooperation. Lessons so far include a recognition that CI assessment requires a new mindset that is difficult to acquire and maintain. Long and sometimes inefficient discussions are frustrating, but may be necessary when learning to use flexible problem-solving procedures to address complex problems. There are data shortfalls that are likely to persist, and analytical procedures are constrained by available information. Collaboration with other institutions and agencies is imperative but the lack of preconceived directions and perceived uncertainty of people who are trying to guide the CI assessment procedure may hinder broad and enthusiastic involvement of other interests. Stronger commitments to the CI process from all interested institutions is needed.

Upper Mississippi River Recreation Economics Study

The Upper Mississippi River Basin Commission (UMRBC) is comprised of five states and several federal agencies. The UMRBC recommended to congress that a Recreation Economic Study (RES) be conducted to estimate the value (economic benefits) and economic impact of recreation on the Upper Mississippi River. A long history of interaction between concerned organizations has established well-developed institutions including the UMRBC and the Upper Mississippi River Conservation Committee (UMRCC). This latter organization and the five states involved have strong preferences for emphasizing the economic impact elements of the RES established by Congress. Traditional CI cost/benefit studies do not emphasize economic impacts, however, and an impasse had developed regarding how the RES should be conducted. NEC/FWS joined a cooperative agreement with a ACE (St. Paul District) to develop a Plan of Study (POS) to guide the RES. Development of the POS was viewed as a CI

project because of obvious CI elements connected with use of the Upper Mississippi River. For example, development of recreation facilities such as boat docks and water access may affect fish and wildlife resources as well as local and regional economies. Alternative uses of the river such as increased barge traffic, resulting from enlargement of locks and deepening of channels, would also have both economic and ecological implications. Impacts from such development activities could accumulate in either simply additive or highly interactive (synergistic) ways, and along numerous biological and institutional pathways.

A collaborative workshop was conducted by the CI project team of FWS/NEC. Economists, administrators, ecologists, and recreation planners representing state, federal, and regional institutions reached agreement on the relative importance of economic impact analysis and economic benefits, and made specific recommendations about which of several alternative economic analysis methods and data bases should be used in the RES. The results of that meeting led directly to a plan of study (POS) that has been accepted by the necessary parties and has begun to be implemented. The most significant reasons for that success are that a common starting ground had previously been established by the Congressional Act requiring an RES; the appropriate people attended the collaborative workshops, including disciplinary specialists (economists, recreation planners, and ecologists) who could resolve differences of substance or misunderstandings; and the format of the workshop had as its central method a clear, objective articulation of the issues and achievement of consensus.

This case study also provides a good example of needed collaboration between various operational elements of the same organization or institution. For example, river activities that enhance recreation values may serve general natural resource enhancement purposes well, but some of those same activities are not neutral to the enhancement or preservation purposes of some specific fish and wildlife species. More positively, however, it was also concluded that the activities of traditionally development-oriented agencies and organizations can be carried out to develop recreation resources and enhance wildlife as well as to improve barge traffic. This case includes a lesson that economic analysis may not always be hostile to ecological objectives, and in fact may at times justify natural resource enhancing objectives. Similarly, this case opens the issue that it is not just ecological impacts that can accumulate; the economic and social implications of alternative actions and policies may accumulate as well. Sometimes these non-ecological impacts will be an important part of cumulative impact assessment processes.

Ecological Risk Assessment

The Ecological Risk Assessment (ERA) Project is an interagency agreement between the FWS/NEC and the Office of Toxic Substances (OTS) of the Environmental Protection Agency (EPA). EPA is the major funder of the project, and is directing the project toward the development of pragmatic methods for use in reviewing new chemical compounds that are proposed for production and use. FWS interest in the work is primarily motivated by several CI studies suggesting that environmental or biological thresholds are needed before CI can be addressed. This is especially important when neither

clear regulatory jurisdiction or societal objectives are available for the scale at which serious CIs are operating. In those cases, ERA might be used to estimate the probability that given target resources will decline beyond some (alternative) level as a result of proposed actions. Trade-offs between project benefits and the probability of negative environmental consequences may then be negotiated more honestly. When objectives are known, but dose-response functions are not, ERA may be used to estimate the probability that the goal will be precluded as a consequence of proposed actions.

A ecological risk assessment (ERA) colloquium was conducted in November, 1986 to design ERA procedures for OTS. Target resources used in regulatory processes are normally high level carnivores or economically important species, but many of the consequences of resource development or chemical production activities occur at much lower trophic levels. A principal objective of the colloquium therefore was to integrate models that are meant to explain cause and effect relationships at several biological levels. Three computer models were linked together to estimate the probability that a chosen species population would decline beyond some target level as a consequence of exposure to various concentrations of a chemical compound. A unique procedure termed the optional risk procedure (ORP) was also developed to aid decision makers in choosing which chemicals to study in greater detail.

Collaboration in this project was good between the FWS and EPA and the colloquium also included involvement of the regulated chemical industry. The most valuable interaction, however, was between OTS regulatory scientists who insured pragmatic utility in colloquium recommendations, and other scientists who used existing computer models to improve the regulatory decisions made in OTS. A conceptual paradigm was also developed that will improve the ecological models and decision support systems available at the colloquium. A follow-up workshop was held to incorporate the results of recommended research and development efforts completed since the colloquium. The optional risk procedures were improved during a subsequent meeting.

The major contribution of the ERA project to CI assessment was the development of a procedure for estimating thresholds and for observing how the consequences of perturbations approach those thresholds. The development of simplified graphics techniques in the ERA project will also be useful for making trade-offs and negotiating compromises in CI assessments. An ancillary contribution of the ERA project was to demonstrate that the collaborative problem solving approach being used to develop a CI process is achieving some success in developing ecological risk assessment procedures.

Fish and Wildlife Service CI Projects in Alaska

The U.S. Fish and Wildlife Service is developing a guidance manual for predicting and evaluating the impacts of proposed oil and gas developments on the North Slope of Alaska. A CI assessment was conducted as part of that project. It was found that inadequate coordination and planning between developers and insufficient guidelines from appropriate federal and state agencies made CI assessment difficult. The manual documents that secondary physical impacts, as indicated by increased thermokarst, are occurring at Prudhoe Bay. These impacts can be related to the primary direct impacts of

road, pad and facility construction. In addition there are cumulative impacts of oil development on shorebirds at Prudhoe Bay. It was found that the density of breeding shorebird populations is affected by the density of oil field facilities.

Further development of Alaskan oil is anticipated, and FWS may apply the FWS/NEC CI process to those case studies in the future. The Alaskan oil development cases provide an opportunity to study CI from one type of development on a well-defined spatial scale and on relatively undisturbed fish and wildlife resources. The Prudhoe Bay study has already demonstrated how secondary effects can be managed in a CI assessment.

Great Lakes

Roughly half of all coal used to produce electricity in the U.S. is burned in the Great Lakes area, and half of the toxic waste sites identified so far by the Superfund are located there. It is believed that the use of approximately 900 chemicals and heavy metals has led to reductions in wildlife populations. A CI study being initiated by the FWS/NEC project is addressing a small subset of Great Lakes problems over a restricted spatial scale. An initial FWS scoping meeting narrowed the scope of the study to the St. Clair and Detroit waterways. It is expected that the scope of work will be expanded if this initial project is successful.

Most of the fish and wildlife resource agencies of Michigan and Ontario will be involved in future meetings and workshops. An existing geographic information system (GIS), called the Great Lakes Information Management System (GLIMS), will be modified and converted from a mainframe to a microcomputer format. GIS data and other existing information from state, federal and regional institutions will also be utilized. Agreement will be sought regarding observed wildlife population decline, major causal pathways for those reductions, and feasible remedies. Quantitative proportions or numerical coefficients for cause/effect relationships have not been attempted.

Institutional mechanisms for making decisions and implementing actions seem available for many previous Great Lakes issues, and consensus developed through collaborative problem solving may be adequate for bringing about required changes in resource management, development and restoration policies. The Great Lakes CI study can contribute to general CI assessment knowledge by investigating the implicit hypothesis that a rich history of complex problems has led, through improved technical understanding, to the development of institutions capable of making broad scope decisions in a relatively short time. Investigating this hypothesis, through the Great Lakes CI study, can identify the adjustment mechanisms of institutions as they change to match complex problems that supercede common institutional jurisdictions. Research, development and policy can emphasize enhanced technical understanding with the expectation that appropriate institutional adjustments will follow. In that scenario, studies to illuminate how institutional capabilities and remedies evolved in the Great Lakes should be undertaken so that other interstate and international problems can benefit from that experience.

If no institutional mechanisms are found to design and implement solutions to CI problems in the Great Lakes, then an R&D emphasis should be directed toward careful studies of institutions and how they may be directly influenced toward more authority and accountability for problems and solutions beyond any current agency jurisdiction.

Glacier National Park

Fifty-six external threats to the ecology of Glacier National Park were identified in 1980. The North Fork Basin of the Flathead River was identified as being particularly sensitive to external land use activities. A problem analysis technique patterned after the FWS/NEC CI process was used to evaluate the cumulative effects of external activities on resources within this region of the Park (Raley et al., 1987).

A cause and effect (C/E) analysis was conducted during a three-day workshop held at Glacier National Park. A problem statement representing the environmental issues was identified, cause and effect relationships were determined, and tasks to prevent or remedy the problem were defined. The C/E analysis allowed participants to take an unrestrained view of the situation, and models were able to address simultaneously many resources and concerns. This was viewed favourably by the National Park Service (NPS) analysts, because the North Fork Basin represented ecosystem-level problems that needed to be addressed.

The C/E analysis was judged by the NPS as being appropriate and useful for potential or chronic problem analysis, but perhaps too cumbersome for short-term analysis of acute and urgent problems. When problems arise requiring immediate action there may not be sufficient time to contact the necessary experts and arrange a meeting for collaborative problem solving.

The Glacier National Park study provides an example of CI assessment when agency goals are well established through a management plan, and single-agency jurisdiction is paramount.

Alaska National Parks

The NPS is developing a process to assess cumulative impacts of placer mining for gold in three NPS units in Alaska. These CI processes are needed in support of environmental impact statements, mineral management plans and park master plans. The NPS is in the very early stages of process development, and is currently investigating a modified version of the LAC process. No indexing or weighting of resources or impacts would be attempted, and explicit interactions between mining developments would be ignored. This process relies on avoidance of long-term impacts beyond LAC levels from any individual project, thereby frustrating measurement of the accumulation of negative impacts in excess of threshold levels. The LAC process is in the early stages of development, and decisions about its characteristics and implementation are pending.

7.3.3 Some Conclusions

The U.S. Council on Environmental Quality (CEQ) explicitly included consideration of cumulative impacts in CEQ 1978 regulatory policy. Several federal and state agencies have initiated CI assessment procedures and conducted case studies of CI since that time. The U.S. Fish and Wildlife Service is developing procedures to assess cumulative environmental impacts on fish and wildlife resources. Scientists, managers and policymakers are using collaborative problem-solving techniques to design, develop and implement CI assessment processes. Twelve case studies were conducted or observed during the FWS CI project. These case studies verify some principles that were anticipated at the beginning of the project, and lead toward some new conclusions regarding CI assessment.

- 1) Cumulative impacts are highly complex and variable. Attempts to assess them must be comprehensive and flexible. This is best achieved through a general collaborative problem solving approach where detailed processes are developed somewhat spontaneously to solve specific CI problems.
- 2) CIs are characterized by complexity where existing analytical tools and institutional jurisdictions do not match in terms of the temporal and spatial scales at which serious impacts accumulate. This means that more than one method and institution will frequently be needed to reach viable solutions. New linkages between available methods and among existing institutions are needed.
- 3) Collaboration at all levels, throughout the CI project has been a central theme for this work. Collaboration vertically within organizations, among disciplines and institutions is needed for effective CI assessments. Active participation throughout the design, development and implementation phases of specific CI assessments improves results, hastens technology adoption, and trains practitioners to work collaboratively on complex problems.
- 4) Collaboration is difficult to attain and is inherently fragile in many bureaucratic environments. Substantial autonomy from organization line control may be needed, and close teamwork by core team members is essential. Changes in supervisory personnel and management authority can compromise these essential attributes; previous commitments become less secure, understanding of the process development strategy is weakened and normal bureaucratic power struggles accelerate. When high-risk development projects having high potential pay-offs are undertaken, sponsoring agencies need to work toward a stable funding and supervisory commitment of three to five years duration. Involvement of high-level administrators, as well as operational users, needs to be firmly established. This is not to provide an implicit threat that if the preferences of either group are perceived as being temporarily unmet, support and involvement

will be withdrawn, but for the very legitimate reason that broad new processes cannot be developed without that involvement.

8.0 APPENDIX ON CEA METHODOLOGIES

8.1 Annotated List of Potential CEA Methodologies

The following sections provide: (1) a brief description of nine methods for environmental impact assessment, (2) comments on the potential usefulness of each method type for cumulative effects assessment (CEA), and (3) a conceptual outline of the major steps involved in using each of the methods suitable for CEA.

Of the nine techniques for environmental impact assessment, the first eight were identified by Shopley and Fuggle (1984). The nine analytical methods are:

- 1) ad hoc;
- 2) checklists,
- 3) matrices,
- 4) networks,
- 5) mapping and overlays,
- 6) modeling,
- 7) weighting/evaluative methods,
- 8) adaptive procedures, and
- 9) biogeographic theory.

1. Ad Hoc

Overview

In this classification, ad hoc refers to those methods that rely primarily on expert judgement and do not structure the problem to make it amenable to systematic analysis. Included in this classification are guidelines that suggest possible impacts without recommending specific means for their measurement or evaluation. Another type of ad hoc analysis, "pure" expert judgement, is characterized by a process of assessment that cannot be replicated. Each expert's conclusions are based on a unique combination of experience, training and intuition. In some assessments this is the only required or possible approach. In other instances, when more scientific methods are available, it is not sufficient to rely on ad hoc methods.

Detailed Review

Commonly, ad hoc methods are not applicable outside of their originating agencies, and many other environmental impact assessment methods were developed due to dissatisfaction with these methods. Because ad hoc methods

are considered inappropriate for most environmental assessments, it is unlikely that they will be applicable to the compounded difficulties associated with cumulative impact assessment. However, ad hoc methods can be useful for helping to organize information prior to applying other methods.

Ad hoc methods have been used to evaluate energy projects in California, Colorado, and Maryland as well as offshore oil and gas development. Because the stages associated with these methods vary greatly, no attempt was made to set forth an outline of the major steps.

2. Checklists

Overview

Checklists are standard lists of potential impacts associated with a particular type of project. Checklist methods are primarily for organizing information or ensuring no potential impact is overlooked. In one sense, checklists are a more formalized version of ad hoc approaches in that specific areas of impact are listed and instructions are supplied for impact identification and evaluation. More sophisticated checklists (Canter, 1977) include scaling checklists, in which the listed impacts are ranked in order of magnitude or severity, and weighting scaling checklists. In weighting scaling checklists numerous environmental parameters are weighted using the expert's judgement, and then an index is calculated to compare project alternatives (Stover, 1972).

Westman (1985) lists some of the problems with checklists when they are used as an impact assessment method: (1) they are too general or incomplete; (2) they do not illustrate interactions between effects; (3) the number of categories to be reviewed can be immense, thus distracting from the most significant impacts; and (4) the identification of effects is qualitative and subjective. Checklists make no attempt to assess impacts. Because of these limitations for environmental assessment, checklists are not likely to be very effective for cumulative impact assessment.

Detailed Review

For the most part, checklist methods offer little potential for cumulative impact assessment; nevertheless, they are useful for helping to organize information prior to applying other methods.

3. Mapping and Overlays

Overview

Shopley and Fuggle (1984) credit McHarg (1969) with original development of map overlays, although this method has also been used extensively by others (Lewis, 1976). An overlay is based on a set of transparent maps, each of which represents the spatial distribution of one environmental parameter such as susceptibility to erosion. Overlays are the fastest way to identify zones which have all of a given set of variables. To investigate the degree of associated impacts, any number of project alternatives can be located on the

final map. The validity of the analysis is related to the type and number of parameters chosen. For a readable composite map, the number of parameters in a transparency overlay is limited to about ten.

These methods are used in at least two ways in cumulative impact assessment. One way is to use before and after maps to give a visual assessment of changes to the landscape. The other way is to combine mapping with an analysis of sensitive areas or ecological carrying capacity. When used in the latter manner, constraints on the level of development are set on the basis of limits determined by the location of sensitive areas and by assessments of carrying capacity. These methods are spatially oriented and are capable of clearly communicating the spatial aspects of cumulative impacts.

The limitations of mapping and overlay methods relate to lack of causal explanation of impact pathways and inability to predict population effects. As well, these methods are unable to deal with large numbers of variables and cannot differentially weigh the relative significance of different variables. However, some sophisticated versions can make predictions about potential habitat loss.

Geographic information systems (GIS) are an extension of the mapping/overlay concept using computer technology. GIS may also be linked with computer models.

Detailed Review

We evaluated three methods based on mapping or overlay techniques. In all cases mapping was combined with some other method for determining environmental sensitivity or carrying capacity for a given spatial area. Based on this information, limits were placed on the scale and type of development.

In developing the analysis in support of a general permit for the lower Colorado River, the U.S. Army Corps of Engineers, Los Angeles District (1982), conducted mapping and an inventory of environmental and cultural resources in each of a number of river segments. The resources considered included: water quality, aquatic biological resources, terrestrial and wetland resources, cultural resources, land use, demographic and socio-economic considerations, and recreation and public safety. Proposed general permit areas are delineated on the basis of calculated resource sensitivities/impact relationships over the entire river.

This method only partially satisfies the problem definition criterion. The main drawback is the lack of explicit consideration of the causal basis for cumulative impacts. While not a predictive tool, nor a tool for explicit aggregation of cumulative impacts, this method does consider and define thresholds for development in each of the river segments. The production of an atlas containing maps of environmental and cultural resources and designated development areas clearly aids in communicating results. Depending on the level of resolution required, information is relatively easily obtained; however, some river segments will have better information than

others. If extensive resource inventories are required, or a detailed Geographic information system is necessary, this method would likely become costly. Regulatory agencies that normally participate in the review of permits may suggest that general permits do not ensure adequate protection from cumulative impacts.

Winn and Barber (1985) looked at the cumulative impacts on grizzly bears by linking a computer simulation model with a geographic information system. The geographic information allows production of computerized maps of grizzly bear habitat and concentrations of grizzly bears. By superimposing road and campsite developments, changes in the amount of habitat could be calculated. Habitat changes were then used to estimate impacts on grizzly bears. While this application is simplistic, linking mapping techniques to computer simulation modeling techniques may be applicable in a large number of cases.

This method has been used to assist in habitat management for grizzly bears in several U.S. federally managed areas including the greater Yellowstone ecosystem and the Shoshone National Forest. To use the method as described by the authors, it is necessary to have detailed geographic information on vegetation and locations of grizzly bear activity, as well as "displacement coefficients". Displacement coefficients refer to the degree to which a particular activity will result in avoidance behaviour on the part of grizzly bears.

This method only partially satisfies the criterion for defining cumulative impacts since it fails to explicitly consider the causal basis for cumulative impacts. This is partly due to the nature of the application described. This method also fails to satisfy the criterion as an assessment tool. However, the discussion of the method and the proposal to link a geographic information system with a computer simulation is of interest. The results are relatively easy to communicate. The method can be costly if computer needs for the geographic information system and the simulation models are great.

Dickert and Tuttle (1985) used information collected in field studies to determine sensitivity to erosion, relative effects of different types of land disturbance, and a "land disturbance target" to assess cumulative impacts in Elkhorn Slough, California. Target values were based on the amount of land available in low erosion zones or on maintaining sedimentation rates at long-term averages. Future projects will be evaluated based on whether they violate the defined targets. The analytical work supporting the method consisted of four components: hydrological assessment of runoff and sediment transport, field measurements of erosion and deposition resulting from various land uses throughout the basin, photogrammetric analysis of upland and wetland change spanning a fifty-year period, and measurement of site disturbance associated with dominant land use.

Dickert and Tuttle's method is designed to deal with cumulative effects problems in coastal wetlands. Although their case study involved work done at Elkhorn Slough, California, the authors suggest this method would be applicable to problems involving wildlife habitat, agricultural land conversion, visual quality, and geologic hazards. This method requires that

the cumulative effect problem be visible on aerial photographs, and that a significant length of record be available.

Of the mapping methods, this method appears to be better at defining the causal basis for cumulative impacts, although there is limited consideration of the biological impacts. It explicitly considers thresholds and integrates impacts over space and time. It appears to be easy to communicate, especially through the use of aerial photographs. Obtaining a time series of aerial photographs may be costly or impossible, however.

4. Matrices

Overview

In this classification, we consider interaction matrices. An early example of this method is the Leopold matrix (Leopold et al., 1971). In a Leopold matrix, and its variants, the rows of the matrix correspond to project actions (for instance, flow alteration) while the columns represent environmental conditions (for example, water temperature). The impact associated with the action row and the environmental condition column is described in terms of its magnitude and significance. The Leopold matrix represented the first attempt to systematically relate project actions to changes in environmental conditions.

Leopold-type matrices have been criticized because they are only appropriate for identifying first order interactions. To overcome this deficiency, the extended component interaction matrix (Bisset, 1980) was developed. This method advocates development of a second matrix that accounts for second and higher order impacts by identifying relationships between the environmental components.

Most matrices were developed for specific applications, although the Leopold matrix itself is quite general. Early in an assessment, an interaction matrix could be built for a specific cumulative effects problem. In such a situation it would be a useful tool for identifying interactions between project activities and specific environmental components. It would also be useful for identifying interactions between environmental components. However, in terms of problem definition, matrices tend to overly simplify impact pathways. Also, they do not explicitly represent spatial or temporal considerations, nor do they adequately address synergistic impacts.

Thus, as an assessment tool, matrices can only provide qualitative predictions of the impact a specific action may have on a specific environmental component. Since no two action-component interactions on any one matrix can be precisely compared, attempts to integrate across activities or environmental components are difficult. In general, the technique is easy to use only if one has either knowledge of the assessment situation or expertise from similar situations. Also, because of the sheer size of a typical matrix covering virtually all project actions and affected environmental components, the matrix becomes cumbersome. Consequently, matrices are probably best used for the purpose for which they were designed; that is, as tools for preliminary analysis and screening in environmental impact assessment.

For cumulative impact assessment the usefulness of matrices is limited to helping determine the set of activities that may affect a given environmental component. If these techniques are to be used, it is necessary to develop a new matrix for each new application or to use a tried and true matrix for a similar application.

Detailed Review

For the most part, matrix methods offer little potential for cumulative impact assessment; nevertheless, they are useful for helping to organize information prior to applying other methods.

In Canada, the Federal Environmental Assessment Review Office (FEARO) published a guide for environmental screening (FEARO, 1978) that presented two sets of matrices designed to assist in identifying potential adverse effects associated with proposed projects. The matrices were designed to assess effects from single projects, and gave no explicit directions for evaluating a project in context with other developments and activities.

5. Networks

Overview

The stepped matrix technique, developed by Sorenson (1971) to display the possible consequences of land use along the California coastal zone, illustrates how the matrix approach evolves logically into network diagrams. Network diagrams can be constructed from matrices where matrix elements represent linkages between pairs of components. These network diagrams provide the mechanism for linking first and higher order impacts. System diagrams have been used by ecological modelers to represent the conceptual structure of models. In the context of environmental impact assessment, one group of modelers, for example, used a sophisticated network, or system diagram, to represent impact hypotheses (Everitt et al., 1986) which causally related project activities to target resources.

For problem definition, networks or systems diagrams overcome the limitations of matrices by accommodating higher order impacts. They are also far better at explicitly identifying the causal basis for impacts. In addition, they are well suited to identifying the interaction between a number of activities, components, and a single target resource. As an assessment tool they are capable of making qualitative predictions regarding the cumulative impact of a number of activities on a single target resource. They do not formally integrate, however, over the spatial and temporal dimensions, nor do they integrate across target resources. Networks and systems diagrams can be communicated well and are easy to develop using expert judgement. Scientific documentation of complex systems diagrams, however, requires time and money.

Detailed Review

We looked at three methods representative of this group. In general, these methods represent cumulative impacts as causal relationships embodied in a

conceptual model. In some cases network diagrams are used, while in more sophisticated cases computer models are developed.

The stepped matrix approach of Sorenson (1971) is excellent for hierarchically describing the relationship between activities, impacts, and potential mitigation measures. It provides for linking many activities to a single impact category, while providing for limited detail on the important ecological relationships. This method, originally developed for coastal zone impact management, could be adapted for use in other situations.

The stepped matrix approach provides a combination of a stepped matrix and columns network which enables identification of:

- 1) land use effects,
- 2) causal factors (land alterations associated with land uses),
- 3) conditions (initial identifiable impacts of causal factors),
- 4) consequent conditions (changes induced by the initial conditions),
- 5) effects (ultimately produced by the consequent conditions), and
- 6) corrective actions or control measures (mitigation).

This method emphasizes interrelationships among 1-6 and includes impact risk and mitigation measures. This matrix network is the starting point for further examination of interrelationships, information indexing, forecasting, and evaluation. The stepped matrix approach can be computerized, allows for non-quantifiable impacts, and treats multiple-use issues.

The method partially satisfies the criterion for problem definition. In some applications, such as where temporal aspects were explicitly considered and impact pathways were clearly shown in network diagrams, the method should perform well at problem definition. Although the method describes qualitative interactions, it does not represent synergistic or aggregative impacts. The method also does not explicitly consider thresholds. It does, however, make qualitative predictions of cumulative impacts across activities. The method is easy to explain, and the matrices facilitate a clear exposition.

6. Weighting/Evaluative Methods

Overview

In Shopley and Fuggle's (1984) classification, evaluation methods ensure that impact assessment is based upon acceptable value judgements. Most of these methods involve the determination of scales and assignment of weights that reflect people's values or preferences. Therefore, these methods are best applied during the assessment of alternatives, especially when choices are being made between alternatives. Westman (1985) provides a good description of various types of evaluation methods.

Evaluation methods must normally be linked with other methods (Lewis, 1976) and are applied after the impacts have been analyzed. These methods are not used to make predictions about impact magnitudes, as measured by changes in levels of target resources. A simple evaluation method would compare impact levels with some predetermined standard or threshold. More sophisticated methods for weighting the relative significance of various impacts aggregate them into indices that can then be used for comparing alternatives. These are the only methods that attempt to integrate across a number of impact dimensions or target resources. In most cases, the methods will determine that alternative A is preferable to alternative B; they cannot, however, indicate whether either alternative is good or bad.

Detailed Review

We have evaluated two methods that compare alternatives based on weighing preferences or expert judgement. These methods assume that information is available on the measurement of impact for a given project or alternative. Their strength is that they aggregate over a number of different impact dimensions to give an overall index or rating that can be used to compare alternatives.

Anderson (1981) proposed a general method involving evaluation of preferences between various groups which he calls "affected publics". Consistent scales are constructed to measure each of the impact dimensions. Relative weights are then assigned by the various publics to each of the impact dimensions. Using these weights and measurement scales, an index is constructed for each public and each alternative. In the full method, weights are assigned to each of the publics and then an overall index or score is assigned to represent the aggregate public preference for each alternative. This method relies completely on subjective judgement, and any applicant must decide who has a legitimate right to weigh one environmental resource against another, or trade off one affected public against another.

The key concept in Anderson's method is called commensuration. Commensuration is the process of determining what impact on one dimension is equivalent to a given impact on another dimension, or what impact on one public is equivalent to a given impact on another public. The detailed steps in the method are:

- 1) identification of impacted publics,
- 2) identification of issues (for instance, dimensions of impact using checklists, and ordering of dimensions in terms of importance),
- 3) measuring values in terms of functional curves (public representatives judge the functional forms), and
- 4) measuring weights (weight judgements are the basis for commensuration in the first step of the cascade, one weight per dimension per public; weights are measured by ratio scales).

Full commensuration is achieved in two cascaded steps: (1) across dimensions within each public, and (2) across publics. Within each public,

commensuration yields the average evaluation of each alternative on each dimension, and the average evaluation by the public of the overall value of the future with and without the project. Commensuration also incorporates the value of mitigative measures for each project alternative. If a single best alternative is still not apparent, then the problem of trading off one public's loss for another public's gain is addressed via a single "trade-off dimension". The method employs various equations, aggregations and weighted summing techniques, and deals with equity, uncertainty, and utility.

To be effective, commensuration requires that representatives of the publics be determined. As with most methods of this type, Anderson's method offers little guidance on how to choose these representatives. The method assumes that the cumulative impacts on target resources have been determined. The method allows for integration across the impact dimension (target resources), something that few of the other methods are capable of doing. The results should be relatively easy to communicate, although they may not be acceptable to those looking for a more scientific basis for assessment of cumulative effects. In cases where the issues are well-defined and the number of affected publics is small, this method could help evaluate alternatives if used by a skilled practitioner. In most applications, however, the method would likely prove unsatisfactory, especially where a wide range of interests must be accommodated.

Bain *et al.* (1985a) propose a method for evaluating alternative project configurations. Their method does not involve trade-offs between publics but rather trade-offs between target resources. The method has a stronger biological basis since it considers key components affecting the impact dimensions. The methodological advance associated with this method is that it allows for interaction between individual projects. This method is explicitly designed to identify cumulative effects over a number of hydroelectric projects proposed in any given river basin.

A key aspect of this method is identification of target resources and components. There are three stages to the method:

- 1) analysis, in which possible interactions between projects are identified;
- 2) evaluation, in which a subset of configurations is selected for more detailed evaluation; and
- 3) documentation, in which the projected impacts are summarized.

The authors suggest using a workshop approach at this stage.

Next, three main types of information must be assembled: (1) component impact values, (2) weights for each impact value, and (3) interaction values. Component impact values are numerical ratings of the impact that each project would have on each target resource component. Weight values refer to a set of weights used when combining component impact values. Interaction values are coefficients to express the interactive effect of each pair of projects on

each component. Each of these three types of information is derived from expert judgement.

This method is used after the environmental impacts associated with each proposed project have been independently analyzed. While it partially satisfies the problem definition criterion, it does not identify the causal basis of impacts, and it considers many of the factors that contribute to impacts on target resources. It is ecologically weak, however, and does not adequately address thresholds. As well it does not predict cumulative impacts; it compares alternative combinations of developments. There are three key limitations for the use of this method: (1) availability and thoroughness of information produced for individual EISs; (2) availability of qualified experts; and (3) the credibility of the experts. The method relies heavily on expert judgement and asks experts to make trade-offs and identify interactions in ways that run counter to normally accepted scientific approaches.

7. Modeling

Overview

Matrices and networks are types of models. They represent the conceptual structure or linkages between the various parts (activities, environmental components, target resources) of a system under study. While conceptual models are necessary to define the problem, they are basically limited to indicating that "x will affect y", or to asserting that activity "a" will cause a minor, moderate, or major impact on species "y".

To move beyond the conceptual level requires quantification. To quantify we must transform the conceptual model into a mathematical model. Mathematical models allow for explicit definition of relationships by allowing the user to specify the shapes of curves that represent the linkages between "x" and "y". For example, we can develop dose-response models for fish toxicity, and we can develop elaborate statistical models for assessing air quality. A special class of mathematical models, such as computer simulation models, are designed for the dynamic representation of ecological systems. When used for impact assessment, computer simulation models can make projections of potential impacts over time. These projections are based on a dynamic representation of the relationships between project activities and the ecological systems under study. Such models are especially well suited to accommodating interactions. Some sophisticated mathematical or computer models can represent synergistic impacts; however, most cannot. Computer simulation models require rigorous problem definition. The conceptual structure of the model, regardless of how it is represented, must be logically consistent. Building the conceptual structure for a computer simulation model appears to be an excellent way to define the cumulative impact problem. For assessment, all mathematical models can be used to make quantitative predictions. Of course, the accuracy of predictions is highly dependent on the quality of the data and on the validity of the model. The structure of ecological simulation models is such that integration over activity, and spatial and temporal dimensions is easily accomplished. Major concerns with computer

models are often focused on the potential for high costs, questions about the accuracy of their predictions, and resistance by some people to accept their use.

Detailed Review

Cause and effect modeling techniques are an excellent method for representing ecological relationships between activities and their potential impacts (Armour and Williamson, 1986; Everitt *et al.* 1986). Armour and Williamson (1986) describe a five step method consisting of:

- 1) stipulating a clear problem statement,
- 2) categorizing causes of the problem,
- 3) modeling causes of the problem,
- 4) categorizing effects of the problem, and
- 5) modeling effects.

When the modeling is completed, causes and effects are linked in a logical network design. These networks provide for causal analysis of the cumulative impacts of multiple actions (causes). The method is an efficient and effective way of categorizing, classifying, and ordering information and ideas into cause and effect relationships. Stipulation of a clear problem statement focusses effort. By constructing network diagrams with groups of experts, the potential cumulative impacts of activities are revealed. The descriptive nature of the networks, however, often does not provide a basis for prediction.

This method appears appropriate for the problem definition phase of cumulative impact assessments. Little attention is paid to spatial and temporal aspects, however, and it appears difficult to represent synergistic impacts. Modeling also provides little guidance on how to assess cumulative impacts. In general, models appear easy to communicate, and they are cost effective to use. Since a model is based on expert judgement it requires little information; however, in a formal assessment a model could be criticized if causes and effects are not carefully documented. In field experiences with models (including barge traffic on the Tennessee River, habitat degradation in Chesapeake Bay, and degradation of Mobile Bay habitat) this method has been generally well received by both the U.S. Fish and Wildlife Service and the U.S. Army Corps of Engineers.

Computer simulation models, like those at the core of the adaptive procedure of Holling (1978a,b), are essential for full development of complex networks and models. Use of simulation allows for integration of impacts over space, time, and activities, as well as providing mechanisms for quantifying thresholds and key ecological processes. Because Holling (1978a,b) is reviewed as an adaptive method, it is not discussed here.

8. Adaptive Procedures

Overview

In the strict sense of the definition, these are not methods but are procedures, or sets of steps, for performing an impact assessment. Adaptive procedures become necessary when no single method is capable of handling all aspects of impact assessment. Within adaptive procedures various combinations of the other seven methods are employed, as needed, at different steps in the procedure.

Detailed Review

Five adaptive procedures that have some potential for use in cumulative impact assessment are:

- Dames & Moore, Inc. (1981)

The most complete set of steps for cumulative impact assessment is outlined in a handbook developed by Dames & Moore, Inc. (1981) to aid the U.S. Army Corps of Engineers personnel in assessment of cumulative impacts of permit activities. First, the proposed activities are identified and the environment characterized. Next, the level of analysis and the assessment approach are chosen.

The approach that relates to the assessment of biological effects proceeds through the following steps:

- 1) identifying effects, using network diagrams;
- 2) quantifying the likelihood, magnitude, spatial extent, and duration of the effects;
- 3) determining the significance of the effects; and
- 4) assessing the ecosystem-level effects.

Each of these steps has a number of detailed substeps. For example, assessment of ecosystem effects has ten substeps, including determining whether synergistic effects are present and whether or not tolerance levels of specific organisms will be exceeded.

In principle, if one followed all the steps and had the tools and resources to conduct the recommended analyses, an excellent cumulative impacts assessment could be done. This method appears excellent for both description and problem definition. As an assessment tool it has potential, for it explicitly instructs users to take account of thresholds, synergistic relationships, and other key ecosystem properties. Its predictive capabilities and ability to integrate across activities and spatial, temporal, and impact dimensions are, however, dependent on the specific method chosen. Because the approach relies on complex sets of specific steps and can include relatively sophisticated methods, it is difficult to understand and use. For simpler applications,

information should be relatively easy to obtain; for more complex applications, the information demands and sophistication of techniques required make this a costly and time consuming method. Given limitations of time and money, it would likely be impractical to use the complete version of this approach. The selection of the method to use at each step is critical, and if approaches like Dames & Moore, Inc. (1981) are to be useful, more guidance is needed on the appropriate method for each step.

- Horak et al. (1983a,b)

Horak et al. (1983a,b) offer a ten step procedure recommended for cumulative impact assessment. These authors emphasize that there is no single method that has been developed to specifically address cumulative impacts; thus, their procedure includes a specific step to decide on the appropriate techniques to be used. The ten steps are:

- 1) examine premises and assumptions that underly the cumulative impact assessment;
- 2) identify and analyze development actions;
- 3) characterize arena (overview of environmental resources and adequacy of existing data);
- 4) scope (determine spatial, political and temporal boundaries);
- 5) map spatial characteristics;
- 6) establish the assessment approach and specific techniques;
- 7) determine characteristics and significance of impacts;
- 8) assess potential ecosystem effects;
- 9) derive some "impression for overall fundamental changes or transformations"; and
- 10) consider subsequent steps for monitoring and reassessment.

They emphasize the need to clearly define premises and assumptions upon which the cumulative impact assessment is to be based. This is necessary because of the confusion that currently exists about cumulative impact assessment and how it should be conducted. Their approach also highlights the importance of selecting the appropriate method; in fact, they make this an explicit step following the problem definition phase.

This procedure is good at defining project activities and the spatial and temporal aspects of the problem. It does not provide specific details on how the methods are to be used. Therefore, it is difficult to know how the approach will perform for cumulative impact assessment. Nevertheless, it does emphasize many of the key attributes of cumulative impact assessments for fish and wildlife.

- Adaptive Environmental Assessment and Management

As already noted, many adaptive procedures do not specify which methods are most appropriate at each step, or how the choice is to be made. One major exception is adaptive environmental assessment and management (AEAM) procedures (Holling, 1978a,b). AEAM uses computer simulation modeling methods in workshops and develops ecological simulation models which then serve as the focal point for the effects assessment.

Holling (1978a,b) advocated simulation modeling workshops as a mechanism to focus and coordinate the assessment team. Whereas the Dames & Moore Inc., (1981) approach outlines steps with little guidance on how accomplish them, Holling suggests that many of the steps can be accomplished by construction of models. Because this approach is based on the principles of systems ecology, and often uses simulation modeling, it is possible to incorporate relationships representing complex interactions and thresholds into the models. The procedure also emphasizes the use of expert opinion, facilitated workshops, and modeling to analyze environmental impacts. It was developed to promote understanding and integration of environmental, economic, and social issues into policy level decisions concerning design and implementation. It should be applied throughout the project development cycle by iterations of modeling or policy analysis workshops alternating with periods of data acquisition, research, and model refinement.

Through computer simulation modeling and other systems analysis techniques, the process: (1) emphasizes interdisciplinary communication; (2) limits the scope of the assessment to key factors; (3) explicitly states assumptions; (4) rapidly synthesizes existing pertinent information and identifies important data gaps; (5) describes integrated system behaviour; and (6) identifies alternatives and promotes collaborative selection. The process is coordinated by a group of four to six facilitator/modelers trained in the techniques of group dynamics, policy analysis, systems analysis, and computer simulation modeling.

Being an ecologically-based approach designed to construct models, AEAM is good at problem definition. The approach formally recognizes the importance of identifying specific project activities, specifying spatial and temporal aspects of the problem, identifying major linkages between system components, and it represents synergistic relationships mathematically. As an assessment tool it has the ability to integrate across activities, as well as spatial and temporal dimensions. It also makes quantitative projections of impact, and can be used to help determine where critical ecosystem thresholds might occur.

Although graphic representation of results helps in communication, it is unlikely that the analytical detail can be communicated to decision makers and lay persons. Models can be constructed based on expert judgement, but obtaining reliable data and validating models is difficult and time-consuming. This approach has had numerous applications in various aspects of environmental assessment (Environment Canada, 1982); generally the response has been favourable, especially when there has been a high degree of participation.

- U.S. Army Corps of Engineers (1983)

The U.S. Army Corps of Engineers (1983) guide to analysis of significance explores ways to define significant impact. The procedures that are advocated examine thresholds in determination of significance. Significance analysis is seen as part of the overall environmental impact assessment process and has five steps:

- 1) identify which resources are significant,
- 2) predict changes in resources,
- 3) define the magnitude and scale of resource changes,
- 4) judge the significance of resource changes, and
- 5) determine the consequences of impact significance.

These five steps are to be preceded by a project description, an assessment of environmental inter-relationships, determination of region-of-influence (for instance, spatial considerations), assessment of the data base adequacy, and description of the environmental setting. The approach concentrates on identifying significant resources (Step 1), judging the significance of changes (Step 4), and determining the consequences to significant impacts (Step 5). The guide itself provides a discussion of questions that need to be answered in the analysis of significance for threatened and endangered species, wetland habitats, unique habitats, fish, mammals, and birds as well as for physical, such as water quality, and cultural, such as Native Indian concerns, resources.

The U.S. Army Corps of Engineers' (1983) approach provides little specific detail on how to define the problem, except that it appears to be based on an understanding of ecological relationships and it tends to define significance in terms of thresholds. Depending on the tools used to determine the magnitude and scale of effects, it may also be predictive. The approach does not explicitly state how cumulative impacts will be integrated over activities, space, time, and impact dimensions. The procedure is relatively straightforward, should be easy to communicate, could be cost-effective in cases where information is readily available; and there is little uncertainty.

- Contant (1984a,b)

Contant (1984a,b) advocates a carrying capacity study as a major part of procedures to forecast and analyze cumulative impacts. In this approach, carrying capacity is taken to mean the maximum level of growth in an area that is consistent with socially acceptable levels of environmental quality and public welfare. A successful carrying capacity study requires agreement on whose values are considered in determining acceptable growth levels, environmental quality, and public welfare.

The procedure, designed for the Corps of Engineers permit process, involves specific steps to predict and monitor project impacts. Steps in the carrying capacity study are:

- 1) identify the area for investigation through scoping of activities and critical problems,
- 2) define the geographic boundaries of the study area,
- 3) determine the limiting factors and related growth activities,
- 4) determine the acceptable level of the potential limiting factor,
- 5) devise a growth variable,
- 6) make assumptions about future types of development,
- 7) devise relationships between activities and growth variables, and
- 8) determine the overall carrying capacity in terms of growth variables.

Different procedures are recommended, depending on the degree of impact and whether or not project impacts are at or near the environmental carrying capacity.

If the scientific and technical aspects of the carrying capacity study are strongly based on ecological considerations, this procedure could be very valuable for problem definition. The procedure emphasizes determination of a threshold upon which to base approval, and the carrying capacity study must make a prediction (most likely quantitative). The concept of carrying capacity is attractive to people, but explanation of the details and assumptions of the assessment may lead to misunderstandings. As well, carrying capacity information may be difficult to obtain and will likely require a costly study. There seems to be disagreement over who should pay for a carrying capacity study; there is reluctance on the part of both regulators and proponents to bear the costs of such studies.

- Summary

Neither the U.S. Army Corps of Engineers (1983) nor Contant (1984a,b) offer a procedure as systematic and comprehensive as those outlined by Holling (1978a,b), Dames & Moore, Inc. (1981), and Horak et al (1983a,b). Both the U.S. Army Corps of Engineers (1983) and Contant (1984a,b), however, focus attention on an important question that cumulative impact assessments must answer: How should carrying capacity be apportioned among projects, and at what point should development be curtailed?

In general, adaptive procedures are considered to be the best approach to addressing cumulative impacts, for they are designed to make the best use of existing methods, available expertise, and information. Their major drawback is that they are time consuming, may be costly, and require considerable coordination of people and tasks. This may make their implementation

difficult. It is clear, however, that these approaches are a response to the complexity and difficulties that people have encountered in assessing cumulative impacts.

9. Biogeographic Theory

Overview

The developing theory of island biogeography (IB), and its associated species-area relationships, offers a possible method for making predictions concerning species losses from isolated patches of ecosystems as a function of incremental encroachments. Such a method would be of obvious value in assessing cumulative effects.

Detailed Review

To date, there appear to have been few attempts to use IB theory in CEA procedures. Nevertheless, island biogeography has been used to describe species losses associated with habitat fragmentation, particularly in forests (for example, Harris, 1984). As well, a computer model has been developed that allows simulation of fragmentation effects on resident species. In this case, species fates were modeled using a species-by-species approach, similar to that described by Gilpin and Diamond (1980; 1982) as "molecular".

Using IB theory to help assess cumulative effects requires that certain types of information be available. For example, island size, isolation from potential sources of colonists, dispersal abilities, dispersal abilities of the species, and population densities. The use of this information for many species and locations will undoubtedly hamper use of this method.

8.2 Description of Loop Analysis

Loop analysis is a qualitative, network technique that is based on feedback relationships. It is a signed digraph type of network analysis. It can be used to predict the changes in standing crops (levels) and turnover rates of the set of variables as they respond to parameter change or forcing from the environment external to the network. The method is based upon the concept of feedback which can be defined as the effect of a variable upon itself acting through intervening variables. Thus, loop analysis differs markedly from most of the network techniques described in Appendix 8.1 which involve straight line causality and little or no predictive capability. Loop analysis is essentially a top-down type of modeling approach in that it deals with the whole structure of the system of interest and allows the assessor to ask what effect does the system have on a particular variable as it is positioned in the network. It also differs markedly from traditional computer simulation techniques in its whole-system focus and proportionally lower effort spent in quantification. In computer simulation, the approach is usually reductionistic, the model is built one link and one variable at a time and much of the total effort is spent on constructing sophisticated differential or different equations, parameter fitting, and model calibration. With loop analysis, the investigator asks: What is the overall structure of the system of interest? Is it stable? What effects will a change in a particular

variable have on the rest of the network? Where are the key control links? Where can stability be enhanced? Once the qualitative structure of the system is understood, it then makes sense to quantify it.

The diagram approach to network analysis has been developed out of engineering (Mason, 1978; Mason, 1952; Lorens, 1956) with varying degrees of development and application in ecology (Saila and Parrish, 1972; Levins, 1973; Hill, 1975). Levins' technique, termed "loop analysis", has been subjected to field tests in freshwater ecosystems (Lane and Levins, 1977; Briand and McCauley, 1978; McCauley and Briand, 1979) and for marine ecosystems (Lane, 1986a,b; Lane and Wright, 1986).

Loop analysis employs signed digraphs to represent networks of interacting variables. The technique allows deduction, from routine monitoring data, of the important variables and interactions in a complex system, such as a coastal marine community. Loop analysis provides a methodology for analysis of systems based on qualitative representations of variable interactions. These are equivalent to the signs of the first partial derivatives of the coupled differential equations describing the system. The models derived from periodic sampling data indicate the dominant variables and interactions, and the predominant driving force (parameter input), for each sampling period. Once the network is constructed, the effects of a parameter input can be assessed in terms of directed changes in standing crops and turnover rates of all variables. Patterns of correlation can be predicted, and the most sensitive components in regard to environmental impact can be identified.

By comparing the theoretical field data, it is possible to construct a restricted set of loop diagrams that represent the system under study. Once the correct representation is determined, the pathways producing the directed changes can be extracted, examined and subjected to a more quantitative form of analysis using standard modeling techniques. Lane (1982; 1986a,b) and Lane and Collins (1985) have used loop analysis for data sets from marine field studies and mesocosm experiments. Lane and Wright (1986) discussed the theoretical basis of the technique.

In the following pages three examples of loop analysis are given. The first is a Type 1, hypothetical loop model not based on data. The second is a Type 2, data-based model using a large routine monitoring data set and the third example (Type 1) illustrates how a hypothetical environmental regulator becomes a co-variable with ecological components.

Advantages of Loop Analysis in Comparison to Quantitative Techniques

A few of the advantages of loop analysis are as follows:

- The qualitative approach gives a high priority to the understanding of whole systems. The models are robust in that they do not depend on precise measurement but mostly on directions of effect.
- Many different types of variables at all levels of scale can be included in one diagram and there is no need to standardize everything into a single unit such as kilocalories of energy or carbon. Variables can be

included which cannot be measured. Human actions such as management decisions or institutions such as regulatory agencies can be diagrammed with algae and grizzly bears.

- Qualitative results are more transferable among ecosystems and new information gained about a given ecosystem can be applied to another area without expensive time-consuming detours to collect additional data.
- Loop analysis also provides a good guide as to what to measure and serves as a design for future analysis. Interpretation of a loop diagram yields important predictions about particular variables in the network, identifies an omitted variable or link and generates testable hypotheses. Data sets are not used to correct equations or achieve better-fitting parameters, but rather to find new ways to look at systems and to find new variables of interest even if they are abstract and impractical to measure. In subsequent research, there can be more selection in collecting the missing information and the "measure everything" approach does not have to be invoked.
- Loop diagrams also serve an important role in assessing environmental impact. Besides the valuable predictions they give, they also provide a useful focal point that is understandable to ecological managers who are trying to make decisions about an environmental problem or proposed development.
- There is a great economy of effort and resources for the amount of information gained in conducting qualitative analysis. If the techniques were developed to their fullest potential, millions of dollars could be saved in unnecessary data collection and impact assessment costs. Because these methods are holistic and enable the investigator to determine the qualitative structure of the ecosystem, understanding of cause and effect is facilitated thereby making much of the contemporary "brute force" data collection obsolete and wasteful. In addition, loop analysis fits into contemporary assessment procedures well. With an adequate set of instructions (procedural manual), government agencies and consulting firms could greatly improve their assessment techniques at less expense and with more understanding of both the impacts and the ecosystems. Note that this manual does not presently exist but it could be developed.

Example of Multiple Effects of a Power Plant on an Aquatic Ecosystem
Using Type 1 Loop Analysis (Hypothetical Model)

Parameter inputs or perturbations to ecological networks are represented by + or - inputs to one or more variables in the network. Natural parameter inputs might include such events as a change in salinity or temperature or an increase in a predator (fish) not included in the diagram, whereas human perturbations include such events as (1) introduction of oil or toxic chemicals; (2) nutrient enrichment, (3) thermal pollution and (4) predation phenomena. The latter category includes fishing, environmental alterations, impingement and entrainment processes among others. One major use of loop

analysis is to assume a parameter input is entering one or more variables and then predict that there will be changes in the standing crops and turnover rates of the variables as well as correlation patterns of these changes and associated levels of variation. Thus, it becomes critically important to determine the relative strengths of natural versus man-made parameter changes for a particular system. For example, how many times has a major industrial polluter gotten "off the hook" by demonstrating there is perturbation in the environment from natural causes. With loop analysis, not only can we predict effects but we can also work backwards: by knowing responses of ecosystems we can identify which variables are the sites of major parameter input. By evaluating the environment and known sources of perturbation by humans, it is often possible to determine which may be more important and whether they are yielding synergistic or antagonistic forces on the ecosystem in question.

In Figure 8.1, a generalized loop diagram is presented for a marine community located near a power plant. In marine ecosystems, we have found that nutrient addition is often an important parameter input that occurs naturally, for example, under spring bloom conditions. With human influences on an ecosystem, there are often multiple simultaneous effects. A power plant will discharge excess heat into the water which will have a positive effect on some algal species (I) and will also have a positive effect on nutrient addition perhaps through enhanced remineralization by bacteria.

At the same time, the mechanical action on the flow of water throughout the cooling system will cause substantial impingement and entrainment of herbivores (H), fish larvae (F_1) and adult fish (F_2). Thus, a single power plant can simultaneously cause five parameter inputs to the ecosystem. In this example, these inputs enhance each other and lead to less N, more I, less E, H, F_1 and F_2 when parameter inputs at I and F_1 predominate (see Table 8.1).

Example of a Data Based (Type 2) Loop Analysis of a Marine Pelagic Community.

To date, loop analysis has been routinely applied to large data sets describing marine pelagic communities (Figure 8.2). These data sets are similar to those collected in biological monitoring programs for purposes of environmental impact assessment. In fitting loop models to data, one of the first procedures involves grouping the raw variables into loop variables. Rare, infrequent species are deleted because there is too little information to characterize them. The data frame involving changes in abundances over time is then transformed into a qualitative data matrix by determining percent relative change of each variable from one sampling period to the next. The loop models are fitted by hand and then checked by computer. A loop model is often finished even though there is not perfect agreement between its predictions and the data. If subsequent attempts to fit it would result in a loss in biological realism, then realism is given priority over accuracy.

A set of loop models is summarized by variables and links to determine the dominant or core network of the ecosystem. The core diagram is a network formed from the most prevalent variables and linkages in a set of individual models. Although a loop diagram for a single date can appear to have missing links, the core structure for an annual cycle or set of diagrams represents

the bulk of important relationships among individual diagrams to be included in the core structure.

A composite core model of several field communities has been developed that is robust and easily transferable among ecosystems. Approximately 300 individual models of marine pelagic systems have been constructed. Environments analyzed to date include St. Margarets Bay and Bedford Basin, Nova Scotia; Narragansett Bay, Rhode Island; Delaware Bay and Long Island Sound. This model includes several hundred species. Few other techniques of ecosystem analysis have the power to treat species individually. The composite core model (Figure 8.2) has approximately 22 variables, 8000 causal pathways and over one million feedback terms. The L variable and its link to Z_2 are shown in dotted lines. They were added after recent analysis of the Long Island Sound data set. Links from Z_2 to adult copepods (Z_1 , Z_3) are given in dotted lines because they are usually connected by one-way relationships which reverse sign and direction frequently. Loop analysis essentially provides the ecological skeleton of an ecosystem by producing the constrained set of variables, links and parameter inputs (driving forces to the networks). The technique greatly reduces the amount of quantification required, since only about 15-25% of all possible links appear to operate in these networks. Successful completion of the analysis also helps verify that variable aggregation has been correct. Loop analysis has also been used successfully with laboratory data sets both for the Dalhousie Aquatron-tower tank and the Marine Ecosystems Research Laboratory (MERL) mesocosms.

Example of the Interaction of Fishing Industry and a Regulatory Agency
(Levins, pers. comm., 1988)

If we want to know the effects of various decisions on the part of the fishing regulatory agency, the decisions may be expressed in terms of simple binary decision making: yes, or no; or the industry can be allowed a certain amount of fishing until some catch size is reached. Whatever the case, as soon as the regulatory agency enters its decision that decision becomes a covariable of the system. The damping coefficient of that moving average will be the result of conflict between those who want rapid response on the part of the regulatory agency and those who would like no interference.

Different schemes of regulation can have different kinds of dynamic consequences. For example, suppose we have a fish population (F) which is self damped, a fishing industry (I) which feeds on it but is not self-limited (will take as many fish and grow as large as it can) and a regulatory agency (X) which reacts according to some fish population index, like the scheme described above. Two possible regulatory procedures illustrate the very different kinds of effects possible. In both cases, as the fish population abundance gets high, then there is no regulatory action: if the fish population abundance is small, then the regulatory agency takes action. Thus X is an effective propensity to act. The two models are given in Figure 8.3.

In Case a, when the agency index is low it acts by putting more fish into the lake, hence, the positive link from X to F. When fish are abundant, so the index is high, then X does not act and so the fish inhibit X and there is the negative link from F to X.

Analyzing this system, we find that no matter what the regulatory agency does to change the fish population, nothing happens because the fishing industry acts as a sink. (A sink in loop analysis means a variable that absorbs all changes and does not pass along input parameter variation to other variables connected to it.)

In Case b, the regulatory agency affects the fishing industry directly. A low level of fish population causes the regulatory agency to inhibit the fishing industry. If the regulatory agency is self-damped in this model as a result of delays in censusing techniques, bureaucratic structure, or political action, then the self-damping term will be small, and it is possible for the regulatory agency to be a source of fish population oscillation and hence uncertainty. Several other effects can be arrived at, but the point here is to demonstrate how human activity is part of the ecological systems and must be incorporated into decision analysis and environmental assessment.

THE USE OF CHEMICAL
DISPERSANTS IN
SALT MARSHES

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THE USE OF CHEMICAL DISPERSANTS IN SALT MARSHES

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Résumé

Nous avons exposé dans ce rapport les résultats d'une étude d'impact sur l'environnement et de l'efficacité de la dispersion du pétrole répandu dans un marais salant situé sur la côte Nord-Ouest de l'Atlantique.

Les impacts du pétrole, du dispersant et du mélange pétrole-dispersant, sur les plantes vasculaires, les communautés algues-bactéries, les propriétés des sédiments et les transformations chimiques du pétrole dans les sédiments ont été examinés. Des expériences ont été réalisées, en parallèle, sur le terrain et dans une serre. L'étude de terrain a eu lieu sur la plage Conrad de l'Anse de Petpeswick, Nouvelle-Ecosse (44°42'N, 63°11'0").

L'étude a été répétée dans des stations établies dans 3 zones à végétation distincte, dénommées zone du bord de l'anse, zone moyenne du marais et zone supérieure du marais.

L'herbe à liens, Spartina alterniflora était dominante dans la zone du bord de l'anse et la zone moyenne du marais alors que l'espèce voisine, S. patens, dominait dans la zone supérieure.

Dans les expériences en serre, des repliquats d'échantillons de chacune de ces zones de végétation ont été récoltés dans le marais salant et cultivés en serre. Dans les expériences de terrain et en serre, un plan d'échantillonnage stratifié du hasard a été utilisé. Des témoins et des échantillons traités par le pétrole, le dispersant et le mélange pétrole-dispersant ont été inclus dans ce plan.

Le pétrole et le dispersant ont été tous les deux répandus à l'aide d'un pulvérisateur portatif après que la végétation du marais ait été mouillée par l'eau de mer. Les effets expérimentaux ont été observés au niveau de la communauté et quand cela était possible, au niveau de l'espèce ou de la cellule. Les hypothèses développées pour les différents paramètres ont été testées par des analyses de variance à deux entrées et le test de rangs multiples de Student-Newman-Keuls.

Les paramètres physiques comprenaient la température du sol et les observations sur sa structure. Les résultats indiquent que dans toutes les zones de végétation, aucun des paramètres étudiés ne fut affecté par les différents traitements.

Les données concernant la chimie des sols étaient très variables, ce qui a rendu leur interprétation difficile. Cette variabilité a été attribuée à la distribution en tâches de la végétation et des détritiques qui pourrait affecter la rétention du pétrole dans les sédiments. Les observations visuelles ont suggéré que l'application du dispersant n'éliminait qu'une faible fraction du pétrole de la végétation. Les concentrations de pétrole dans le sol étaient comparables dans les quadrats et les microcosmes traités avec le pétrole et le mélange pétrole-dispersant, ce qui suggère que le dispersant affecte peu les mouvements du pétrole dans les sédiments.

Les paramètres étudiés pour les plantes vasculaires comprenaient: la taille des plantes, la densité de la tige, le recouvrement spécifique, l'anatomie des feuilles et comme variable physiologique: des mesures de la fluorescence. Les mesures de taille des plantes ont indiqué que le traitement par le pétrole avait relativement peu d'impact sur la taille moyenne dans toutes les zones de végétation. Seul le traitement par le dispersant a causé d'importantes réductions de la taille moyenne des plantes, suivi de près par l'effet du traitement par le mélange pétrole-dispersant. Les fluctuations rapides de la taille moyenne des plantes semblaient être dues à une mortalité plus élevée des petites plantes.

Les données de croissance provenant des expériences en serre, ont indiqué que le pétrole provoquait les plus faibles réductions de taille dans toutes les zones de végétation alors que le dispersant causait les réductions de tailles les plus fortes. Les taux de croissance des plantes traitées avec le mélange pétrole-dispersant étaient généralement plus élevés que les taux auxquels on s'attendait. Il est possible que les mares de pétrole entourant les tissus méristématiques à la base des plantes aient protégé ces tissus du dispersant relativement plus toxique. Les taux de croissance des plantes traitées avec le mélange pétrole-dispersant semblaient redevenir normaux à la fin de la période de croissance, cependant, les grandes augmentations de la mortalité des plantes associées avec ce traitement vont à l'encontre de cet avantage.

La densité des tiges des plantes était la plus affectée par le traitement pétrole-dispersant et la moins affectée par le traitement avec le pétrole seul dans la zone moyenne du marais et du bord de l'anse. Les toxicités relatives du traitement avec le dispersant étaient différentes dans les expériences sur le terrain et en serre. Le dispersant était moins toxique dans les expériences de terrain qu'en serre. Dans la zone supérieure du marais, le pétrole a induit les plus larges réductions de la densité de la tige tandis que le traitement avec le dispersant a induit les plus faibles réductions. La zone moyenne du marais était extrêmement sensible à tous les traitements avec le pétrole et/ou le dispersant avec des réductions de

90% de la densité de la tige associées avec tous les traitements durant les expériences réalisées sur le terrain et en serre.

La production aérienne de biomasse était significativement réduite par les traitements avec le dispersant et le mélange pétrole-dispersant; cependant, les traitements avec le pétrole n'ont pas produit de réduction significative. Les traitements avec le dispersant et le mélange pétrole-dispersant avaient généralement des impacts similaires, cependant, le mélange pétrole-dispersant était légèrement moins toxique dans les expériences réalisées en serre que sur le terrain. Dans la zone moyenne du marais, tous les traitements ont provoqué des réductions significatives de la biomasse aérienne. Dans la zone supérieure du marais, tous les traitements avec le pétrole et/ou le dispersant avaient un impact identique et causaient une faible réduction de la biomasse aérienne. En somme, les traitements avec le pétrole et/ou le dispersant ont produit l'impact maximum dans la zone moyenne du marais et l'impact minimum dans la zone supérieure du marais.

La couverture de Spartina dans la zone du bord de l'anse et la zone moyenne du marais était le moins réduite par le traitement avec le pétrole et le plus réduite par le traitement avec le mélange pétrole-dispersant. Dans l'expérience en serre, les impacts dus au traitement avec le dispersant et le mélange pétrole-dispersant étaient équivalents, tandis que dans les expériences de terrain, le traitement avec le mélange pétrole-dispersant était nettement plus toxique. Ceci pourrait être lié à une interaction inhibitrice entre le pétrole et le dispersant apparue uniquement dans l'expérience réalisée en serre. Dans la zone supérieure du marais, tous les traitements semblaient avoir des impacts de même intensité et ont causé une faible réduction du taux de recouvrement de Spartina. La zone moyenne du marais était la plus sensible aux traitements tandis que la zone supérieure du marais l'était le moins. Salicornia europea était l'unique autre espèce trouvée régulièrement sur les stations étudiées. Son abondance ne fut pas affectée par les différents traitements.

L'examen microscopique des feuilles de Spartina a montré que les mêmes symptômes se sont développés pour tous les traitements avec le pétrole et/ou le dispersant. Le seul symptôme perceptible était la perte de chloroplastes avec le temps. Les taux auxquels cette chlorose avait lieu, variaient avec les traitements, les taux les plus rapides étant associés soit avec le dispersant soit avec le mélange pétrole-dispersant et le taux le plus bas étant associé au traitement avec le pétrole.

Les résultats de la fluorométrie ont indiqué qu'une fluorescence variable était un bon prédicteur des réponses à long terme des plantes soumises aux

stress des différents traitements. Les valeurs prises à intervalles de 100 secondes sont un indicateur plus sensible du stress subi par les plantes que les différentes la variation du maximum de fluorescence. Les premières ont indiqué un stress dans la période d'application du traitement d'une journée alors que ces dernières n'ont varié que quand les plantes étaient sur le point de mourir. Le dispersant seul induisait le stress le plus intense dans toutes les zones de végétation. Le traitement avec le mélange pétrole-dispersant produisait généralement moins de stress que le traitement avec le dispersant. Le pétrole avait été mis en contact avec les plantes avant le dispersant et a peut-être agit comme une barrière réduisant l'entrée du dispersant dans les tissus des plantes. Les traitements avec le pétrole ont causé relativement peu de stress dans toutes les zones sauf dans la zone moyenne du marais. Cette zone était la plus sensible au traitement avec le pétrole et/ou le dispersant et la zone supérieure du marais était la moins sensible.

Les paramètres reliés aux algues ou aux bactéries comprenaient des estimations du degré de recouvrement d'algues ou de bactéries, des mesures de l'activité hétérotrophique et des taux de fixation d'azote. L'abondance des communautés dans le tapis d'algues, celle des bactéries hétérotrophiques et celle des bactéries roses photosynthétiques ont été temporairement réduites par les traitements avec le pétrole et le mélange pétrole-dispersant, mais n'étaient pas affectées ou étaient même stimulées par le traitement avec le dispersant. Les traitements avec le pétrole ont conduit éventuellement à l'augmentation de l'activité des bactéries hétérotrophiques, ce probablement dû surtout à la prolifération des espèces dégradant le pétrole, cependant d'autres espèces comme les bactéries oxydant le sulfure semblaient aussi avoir une réponse positive.

La fixation de l'azote n'était pas affectée par les traitements avec le pétrole et/ou le dispersant dans la zone du bord de l'anse et la zone moyenne du marais. Les apparences suggèrent que ce déclin était causé par du à des altérations du micro-habitat microbial plutôt qu'à une toxicité directe.

En général, les résultats de cette étude suggèrent que la zone de végétation du bord de l'anse et la zone supérieure du marais sont relativement tolérantes aux phénomènes de déversement de pétrole et peuvent probablement s'auto-épurer naturellement. la zone moyenne du marais est, cependant, très sensible, à la fois au dispersant et au pétrole; des mesures de nettoyage alternatives, comme un écoulement vers l'extérieur à faible débit, devrait être utilisées dans des régions du marais où une végétation de ce type domine.

Les résultats des études réalisées en serre et sur le terrain étaient généralement comparables. Les microcosmes des serres étaient faciles à entretenir et pas chers. Leur proximité du laboratoire a permis d'effectuer des observations plus fréquentes et plus détaillées que sur le terrain. Ces facteurs suggèrent que les microcosmes constituent une méthode efficace d'étude des effets du pétrole et des dispersants sur la végétation des marais salants et des communautés microbiennes. Les résultats de cette étude indiquent que la dispersion du pétrole déversé dans les marais salants, en utilisant la technique étudiée ici, n'est pas une mesure viable d'élimination des déversements de pétrole. On a montré que le dispersant (Corexit 9527) est inefficace pour éliminer le pétrole sur la végétation et qu'il est plus toxique pour les communautés de plantes vasculaires que le pétrole seul. D'autres types de dispersant ou d'autres techniques d'application pourraient produire de meilleurs résultats et devraient être recherchés.

SUMMARY

This report documents the results of a study of the environmental impacts and efficacy of dispersing stranded oil in a north-western Atlantic coast salt marsh. The impacts of oil, dispersant, and oil plus dispersant treatments on vascular plants, algal-bacterial communities, sediment properties, and the chemical fate of the oil in the sediment were examined. Parallel experiments were conducted in both the field and in a greenhouse. The field study was conducted at Conrods Beach on Petpeswick Inlet, Nova Scotia ($44^{\circ}42'N$, $63^{\circ}11'W$). Replicate study plots were established in three distinct vegetation zones termed creek edge, midmarsh and high marsh zones. Creek edge and midmarsh zones were dominated by the marsh cordgrass Spartina alterniflora while the high marsh zone was dominated by a closely related species S. patens. In the greenhouse experiment, replicate samples of each of these vegetation zones were removed from the salt marsh and cultivated in a greenhouse. In both field and greenhouse experiments a stratified random design was used. Control, oil, dispersant, and oil plus dispersant treatments were incorporated into the design. Both oil and dispersant were applied by back-pack sprayer following wetting of the salt marsh vegetation with seawater. Experimental effects were observed at the community level and where possible, at the species and cellular levels. Hypotheses developed for the various parameters were tested using two-way analysis of variance and the Student-Newman-Keuls multiple range test.

Physical parameters studied included soil temperature and structure observations. Results indicated that neither was affected by the various treatments in any of the vegetation zones.

Soil chemistry data were highly variable, making interpretation extremely difficult. This variability was attributable to the patchy distribution of vegetation and detritus which may intercept oil, and the variable structure of the sediments which may affect retention of oil in the sediments. Visual observations suggested that little oil was removed from the vegetation by the dispersant applications. Concentrations of oil in the soil were similar in oil and oil plus dispersant treated quadrats and microcosms suggesting that the dispersant had little effect on the movement of oil through the sediments.

Vascular plant parameters included plant height, stem density, species cover, leaf anatomy, and physiological variable fluorescence measurements. Plant height data indicated that the oil treatments had relatively little impact on average plant height in all vegetation zones. The dispersant only treatment caused the largest reductions in average plant height,

closely followed by the oil plus dispersant treatment. Rapid fluctuations in average plant height appeared to be caused by higher mortality among small plants.

Growth rate data from the greenhouse experiment indicated that oil was responsible for the smallest reductions in plant growth rates in all vegetation zones while the dispersant caused the largest reductions. Growth rates for oil plus dispersant treated plants were generally higher than expected. Pooling of oil around the meristematic tissues at the base of the plant may have protected these tissues from the relatively more toxic dispersant. Growth rates for oil plus dispersant treated plants appeared to recover late in the growing season, however, large increases in plant mortality associated with this treatment countered this benefit.

Plant stem density was affected most by the oil plus dispersant treatment and least by the oil treatment in the midmarsh, and creek edge zones. The relative toxicity of the dispersant treatment varied between the field and greenhouse experiments. Dispersant was less toxic in the field experiments than in the greenhouse experiments. In the high marsh, oil caused the largest reductions in stem density while the dispersant treatment caused the smallest reductions. The midmarsh zone was extremely sensitive to all oil and/or dispersant treatments with 90% reductions in stem density associated with all of the treatments in both the field and greenhouse experiments.

Above-ground biomass production was significantly reduced by the dispersant and oil plus dispersant treatments, however, oil treatments caused no significant reductions. The dispersant and oil plus dispersant treatments were generally equal in their impact, however, the oil plus dispersant treatment was somewhat less toxic in the greenhouse experiment than in the field experiment. In the midmarsh zone all treatments caused significant reductions in above-ground biomass. In the high marsh zone, all oil and/or dispersant treatments were approximately equal in their impact and caused relatively little reduction in above-ground biomass. Overall the oil and/or dispersant treatments had the most impact in the midmarsh zone and the least impact in the high marsh zone.

Spartina cover in the creek edge and midmarsh zones was reduced least by the oil treatment and most by the oil plus dispersant treatment. In the greenhouse experiment the impacts of the dispersant and the oil plus dispersant treatments were equivalent, while in the field experiment the oil plus dispersant treatment was clearly more toxic. This appeared to be related to an inhibitory interaction between oil and dispersant toxicity which appeared only in the greenhouse experiment. In the high marsh zone

all treatments were approximately equal in impact and caused little reduction in Spartina cover. The midmarsh zone was most susceptible to the treatments while the high marsh was least susceptible. Salicornia europaea was the only other species which occurred regularly in the study plots. Its abundance was not affected by the various treatments.

Microscopic examination of Spartina leaves indicated that the same symptoms developed in all the oil and/or dispersant treatments. The only noticeable symptom was the loss of chloroplasts over time. Treatments varied in the rates at which this chlorosis proceeded, with the fastest rates associated with either the dispersant or oil plus dispersant treatment and the slowest rate associated with the oil treatment.

Fluorometry results indicated that variable fluorescence was a good predictor of longer term response of plants to treatment induced stress. One hundred second difference values were a more sensitive indicator of plant stress than peak variable fluorescence. The former indicated stress within one day of treatment application while the latter indicated changes only when the plants were near death. Dispersant alone induced the most stress in all vegetation zones. The oil plus dispersant treatments generally induced less stress than the dispersant treatment. Oil was applied to the plants before the dispersant and may have acted as a barrier to entry of the dispersant into plant tissues. Oil treatments caused relatively little stress in all but the midmarsh zone. The midmarsh zone was most sensitive to the oil and/or dispersant treatments and the high marsh zone was least sensitive.

Algal and bacterial parameters included estimates of algal and bacterial cover, measurements of heterotrophic activity and nitrogen fixation rates. Algal mat communities, heterotrophic bacteria and pink photosynthetic bacteria were temporarily reduced in abundance by oil and oil plus dispersant treatments, but were unaffected or were stimulated by the dispersant treatment. Oil treatments eventually led to increased activity of heterotrophic bacteria, probably largely through the proliferation of oil degrading species, however, other species such as sulfur oxidizing bacteria also seemed to respond positively.

Nitrogen fixation was unaffected by the oil and/or dispersant treatments in the creek edge and high marsh zones, however, all treatments reduced nitrogen fixation in the midmarsh zone. Evidence suggests that this decline was caused by alterations of the microbial micro-habitat rather than by direct toxicity.

In general, results from this study suggest that the creek edge and high

marsh vegetation zones are relatively tolerant of single oil spill events and can probably be left to cleanse themselves naturally. The midmarsh zone, however, was very sensitive to both oil and dispersant and alternative clean up measures such as low pressure flushing ought to be utilized in areas of marshes dominated by vegetation of this type.

The results from the greenhouse and field studies were generally very similar. The greenhouse microcosms were inexpensive and easy to maintain. The close proximity of the microcosms to the laboratory allowed more frequent and more detailed observations than were possible in the field. These factors suggest that microcosms provide an efficient method of studying the effects of oil and dispersant on salt marsh vegetation and microbial communities.

The results of this study indicate that dispersion of stranded oil in salt marshes using the application technique studied here, is not a viable oil spill clean-up measure. The dispersant (Corexit 9527) appeared to be ineffective in removing the oil from the vegetation and proved to be more toxic to the vascular plant communities than the oil alone. Other dispersant formulations or application techniques might provide better results, and should be investigated.

INTRODUCTION

Salt marshes are one of the most valuable of coastal ecosystems. As intertidal ecosystems, they provide a high yield of plant material which may be transported by tidal action into coastal waters, where the detritus fuels marine food chains including those leading to commercial fisheries. Salt marshes also serve as nursery areas for larvae and other juveniles of many coastal species. These environments provide protection against coastal erosion. Salt marshes are low energy environments, subject to little turbulent mixing and as such are particularly vulnerable to oil pollution. Floating oil is readily trapped by salt marsh vegetation and sediments, and may remain in the marsh for long periods of time. This persistence is exacerbated by the anaerobic conditions of salt marsh sediments which slows the rate of biodegradation of the oil.

The vulnerability of salt marshes to oil pollution emphasizes the need to develop ecologically-sound oil spill clean up procedures which can be safely used in these ecosystems. Burning, mowing, plowing, and removal of contaminated sod have proven to be ineffective or destructive methods of cleansing oiled salt marshes (Vandermeulen and Jotcham, 1986). Chemical dispersants may provide an alternative method.

Relatively few studies have investigated the effects of dispersed oil on the vegetation of salt marsh ecosystems. Delaune et al (1984) conducted a major study of the effects of oil and dispersed oil on a salt marsh in Louisiana. In their study, dispersants were applied three ways: mixed with water, after oiling of plots; mixed with water and applied alone; and applied to foliage prior to oiling. The oil applications did not reduce Spartina alterniflora productivity in the marsh nor did the dispersant applications. Oil concentration in the sediments was not significantly reduced by the dispersant treatments. A continuation of this study (Smith et al, 1984) demonstrated that there was no long term inhibitory or stimulatory effects of the oil or oil plus dispersant on the growth of Spartina alterniflora.

Baker et al. (1984), reported on a major study of oil and dispersants for a variety of intertidal habitats. Treatments consisted of oil, dispersant, oil plus dispersant, and pre-mixed oil plus dispersant. The dispersants used were BP1100WD, BP1100X, Corexit 8667, and Corexit 7664. Results from

the salt marsh study sites demonstrated a one to two year reduction in the stem density of Spartina anglica associated with both the oil and dispersed oil. Short term loss of Salicornia europaea was also found with both treatments, with recovery after two years. Heavier oils were found to be more toxic than lighter ones, probably killing plants by direct smothering.

Traditionally, the use of chemical dispersants has not been favoured as a countermeasure technique for oil spills in saltmarshes because of concerns that the oil/dispersant mixture or the chemical dispersant alone might be harmful to marsh biota, and that the chemical dispersant would enhance the penetration of the oil into the fine sediments of the marsh (Baker 1975) where it would remain for many years. Recent experiences, however, have suggested that the use of chemical dispersants in saltmarshes may present certain advantages in accelerating their recovery. Experience with oiling of fine to very fine (silt to mud) sediments, such as are found in typical saltmarsh systems, have shown that crude oils and other similarly viscous products treated with dispersants do not readily penetrate into the fine sediments (Vandermeulen 1981). Similar studies with dispersed oil over coarser sediments, in 3.5 m water, have shown little incorporation of such dispersed oil into sediments (Page et al. 1983). Of more direct interest are studies of the biological recovery of oiled marshes that were treated with dispersant. Unpublished reports of follow-up visits to one such oiled marsh (first visited and studied by Vandermeulen and Ross in 1977) indicated that the vegetation of the marsh recovered to normal levels within five years of the spill.

Based on these results, the use of dispersant to clean up oiled saltmarshes may be environmentally advantageous and merits investigation. Considering the virtual absence of acceptable alternatives for dealing with the problem, attempts to expand our understanding in this area can only improve our capability to clean up and protect these very sensitive coastal ecosystems.

This study investigated the impacts of oil dispersion on the macrophytes, microphytes and heterotrophs of a salt marsh ecosystem in Nova Scotia. In addition the chemical fate of the oil and the influence of oil and/or dispersant treatments on physical properties of the sediments were determined. Where possible, experiments were duplicated in both the laboratory where environmental control was possible and in the field where conditions were most realistic.

In order to determine ecosystem level impacts of the oil and/or dispersant treatments this study was conducted at different levels of biological organization. Treatment impacts were investigated at the community level and where possible, at the species, and cellular levels to establish a fully integrated and comprehensive approach to evaluating the biological effects of the treatments.

MATERIALS AND METHODS

FIELD EXPERIMENT

Description of Study Site

The field study site was located at Conrods Beach on Petpeswick Inlet (44°42'N, 63°11'W) (Figure 1). The Conrods Beach salt marsh has developed behind a barrier dune system, and is drained by a single channel which penetrates the dune system. The vegetation of the salt marsh is divisible into three distinct zones, which will be referred to as creek edge, midmarsh, and high marsh zones.

The creek edge zone was dominated by a lush growth of Spartina alterniflora. Small quantities of Salicornia europaea, Plantago juncooides, Suaeda maritima, and Atriplex patula were also present in this zone. The creek edge zone was usually restricted to within two metres of drainage channels in the marsh. The lush growth of Spartina alterniflora was attributable to the relatively well drained and well aerated sediments of this zone.

The midmarsh zone was dominated by a short ecotype of Spartina alterniflora, with lower abundances of Spartina patens, Salicornia europaea, Suaeda maritima, Triglochin elata, Limonium nashii, and Plantago juncooides. This was the most extensive of the three zones in the Conrods Beach salt marsh and was generally found in poorly drained areas in the marsh interior. The stunted growth of Spartina alterniflora in this zone was attributable to the poor aeration of the sediments in these areas.

Spartina patens was the dominant species of the high marsh zone. Small quantities of Spartina alterniflora, Salicornia europaea, Triglochin elata, Suaeda maritima, and Glaux maritima were also found in association with S. patens. The high marsh zone was located on slightly elevated, better drained and aerated soils of the marsh.

Experimental Design

Twelve, 0.5 m x 4.0 m (2·m⁻²) plots were established at random locations in the marsh in each of the three vegetation zones, and four treatments (control, oil, dispersant, and oil + dispersant) were randomly assigned to each zone (Figure 2). Each plot was separated from other plots by a 10 m buffer zone to prevent or reduce cross contamination by either oil or dispersant from other plots. Nondestructive sampling was conducted within

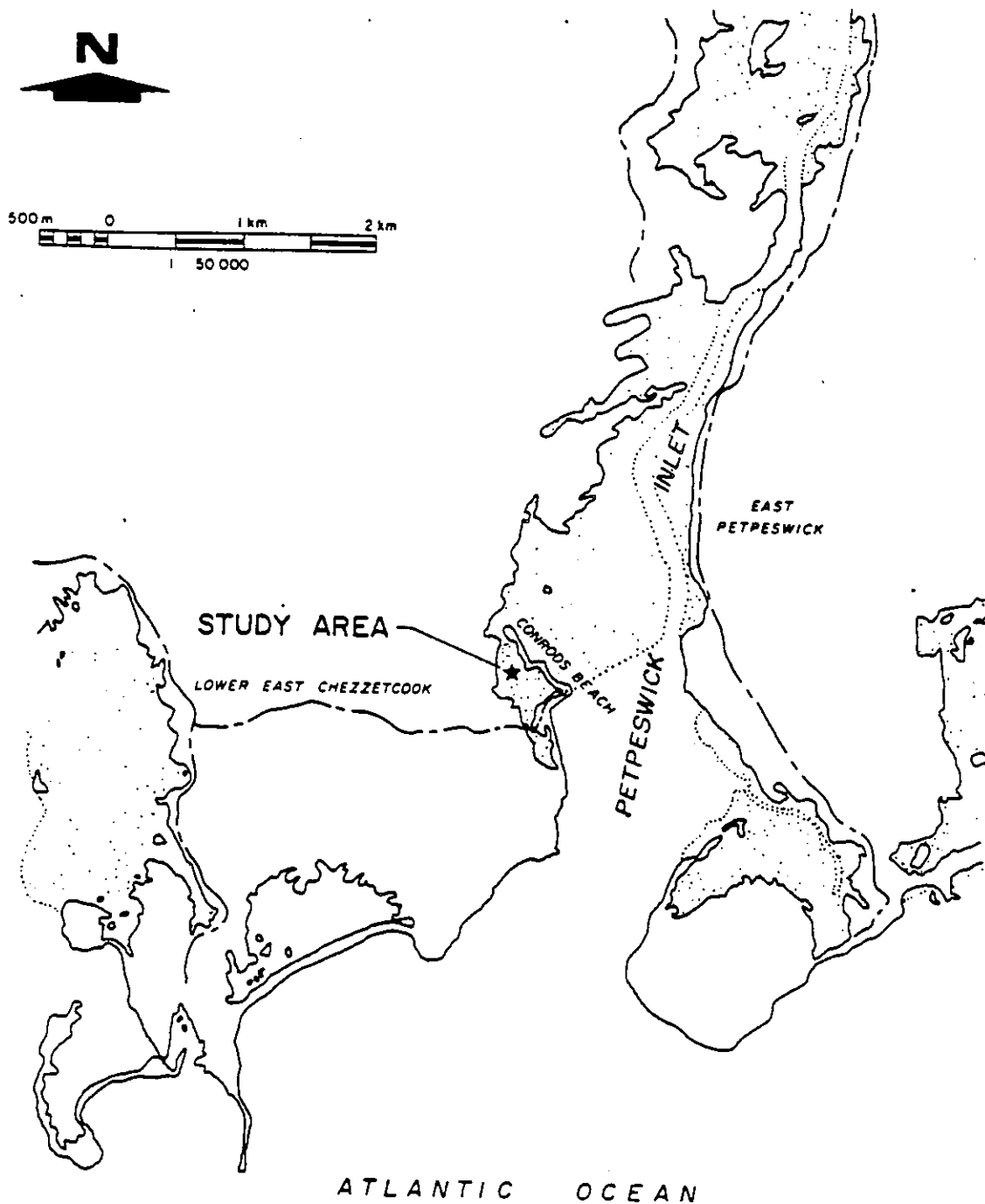


Figure 1: Map showing the location of Petpeswick Inlet and the Conrods Beach salt marsh.

the interior of the plots while destructive sampling was done in a 0.25m^{-2} strip surrounding each plot which received the same treatment as the plot interior. Each quadrat was surrounded by a wall of oil sorbent material to prevent the escape of oil from the quadrat. Quadrats were also covered with garden netting to exclude wildlife. Field measurements commenced on May 9 (day 1) and treatments were applied on July 7 (day 60).

Oil and Dispersant Applications

Alberta sweet blend crude oil, weathered by evaporation and stirring for 24 hours to 15-25% loss by weight, was used for all experiments. In the field experiments, 2.25 liters of weathered oil per plot were applied from a back-pack sprayer approximately two hours after the onset of the ebb period yielding a nominal 0.5 mm oil slick. The tide did not rise high enough to cover the marsh surface with water on the day of treatment applications, therefore, each plot was sprayed with seawater prior to treatment. The oil spray application was done so that the spray was held near the marsh sediment surface, at about half of the mean plant height, thereby simulating contamination by a surface oil slick.

For the dispersant treatment, ethylene glycol-based Corexit 9527 was mixed with water in a ratio of 1 part dispersant to 10 parts seawater. A total of 2.25 liters of the dispersant:seawater mixture was applied to each plot from a back-pack sprayer. Application differed from that of the oil application, in that the dispersant spray was directed at the vegetation from 50 cm above the sediment surface.

For the combined oil plus dispersant treatment, 2.25 liters of Corexit 9527 was prepared as above, but was applied six hours after initial spraying with oil, yielding a 10:1 oil plus dispersant application ratio. Oil was applied near the marsh sediment surface, while the Corexit 9527 was applied from 50 cm above the plants.

Physical and Chemical Parameters

Tidal Analysis - Tidal data were collected over a two week period in order to determine the relative degree of submergence of the quadrats and the elevations of the vegetation groups. An Aanderaa water level recorder (WLR-5) was installed in the main channel, near the center of the test site area (Figure 2). This tide gauge measured the pressure at 5 minute intervals from August 8 to 22. Pressure values were converted into depth values by correcting for atmospheric pressure (pressure data obtained at Shearwater airport, meteorology center of Environment Canada). Water density was approximated at 1.025 g/cc.



Figure 2: Map of the Conrods Beach salt marsh. The location of study plots, tide gauge and sorbent booms are indicated. Numbers associated with plots indicate quadrat code number. C=creek edge; M=midmarsh H=high marsh.

The elevations of the plots relative to the water level recorder were determined using a theodolite. The elevations of the ends and middle portion of each plot were measured and averaged to give the mean elevation for each plot. The level of the water in the tidal creek was measured at regular intervals for comparison with the results of the water level recorder.

Soil Profile Descriptions - Soil profile descriptions were recorded for all cores taken for chemical analysis. On each sampling date, in each vegetation zone, three cores were taken from each of the oil and oil plus dispersant plots and a single core was taken from each of the control and dispersant plots. At each sampling site, a core 6 cm wide by 20 cm long was removed with a modified bulb planter. Each core was split longitudinally and the thicknesses of distinctive sediment layers and presence of oxidizing and reducing zones were recorded on a standard core diagram. The presence of bacterial zones, algal mat communities, the colour of roots, and the degree of root mat development were also recorded for each core. Cores were taken on five dates between early July and early September.

Soil Temperature - Soil temperature was taken at 5 cm depth in the center of each 2 m² plot on 3 dates during the growing season, using a Fisher Accumet Model 640A mini pH and temperature meter. Mean soil temperature values were calculated for each treatment in each vegetation zone on each sampling date.

Soil Chemistry - On 5 dates, using the cores collected above for soil profile descriptions, a 50 g sample was removed from each core and was extracted 3 times with 20 ml of methylene chloride. The resultant solution was dried, taken up in hexane and cleaned up. Samples were cleaned by pouring them through a mini-column containing sodium sulfate, copper and activated florisil. Total oil and grease values were determined with UV fluorescence analysis, scanning from 200 to 300 nm. Chrysene equivalents per dry weight (UV fluorescence at 256 nm) were converted to ppm of oil/dry weight of sediment using fresh and weathered oil samples as standards.

Selected sample extracts were analysed by glass capillary gas chromatography (GC²) using a flame ionization detector (F.I.D.). Relative concentrations of the n-chain hydrocarbon components were determined from the chromatograms. Identifications of the peaks were made using glass capillary gas chromatography mass spectrometry (GC²MS). Procedures followed those of Geiger and Schaffner (1978).

Vascular Plant Parameters

Plant Height - Within each plot, 40 Spartina shoots were systematically selected at 10 cm intervals and their heights were measured. Plant height was measured as the distance from the tallest point of the plant to ground level. Measurements were taken on eight occasions during the growing season.

Stem Density - Within each plot, 12 miniquadrats were systematically positioned at 30 cm intervals. In Spartina alterniflora dominated marsh zones (creek edge and midmarsh zones), 0.01m^{-2} quadrats were used while in the S. patens dominated zone (high marsh zone), 0.006m^{-2} quadrats were used. All shoots in each quadrat and all flowering shoots in each plot were counted and means and standard deviations were calculated for each treatment in each vegetation zone on each sampling date. Measurements were taken on four occasions during the growing season.

Biomass - Biomass harvesting was conducted in the first week of September. A 0.09m^{-2} quadrat was systematically positioned in one end of each plot and all aboveground living biomass in the quadrat was harvested, sorted by species, dried at 80°C in a convection oven and weighed on an analytical balance. Detrital biomass was harvested, dried and weighed but was not sorted by species. All flowering stems in each plot were harvested. Reproductive portions were removed from vegetative portions and weighed separately. Mean above-ground standing crops for living plants, detritus and reproductive tissues were calculated for each treatment in each vegetation zone.

Species Cover - Percent cover for each species of vascular plant was estimated in each plot. Cover values were estimated relative to soil surface area rather than to other species in the quadrat; therefore, total cover could be higher than 100% when species overlapped. Mean cover values were calculated for each species in each treatment in all vegetation zones for 3 sampling dates.

Leaf Anatomy - Two leaves were randomly selected from each plot. These leaves were pooled by treatment within each zone and three were randomly chosen from each treatment for observation. Radial cross-sections were taken from areas on each leaf exhibiting different symptoms of the treatments, and were examined under a microscope at 200x. Observations included the abundance, relative sizes, and coloration of chloroplasts, measurements of cuticle thickness and the presence or absence of plasmolysis.

Microbial Populations

Algal and Bacterial Mat Cover - Color characteristics were used to describe the surface distributions of algal mats (green), sulfate-reducing bacteria (black) sulfur-oxidizing bacteria (white) and photosynthetic bacteria (pink). Percent cover values of algal mat, sulfate-reducing bacteria, sulfur oxidizing bacteria and photosynthetic bacteria were estimated in each plot. Mean cover values were calculated for all categories for each treatment in all vegetation zones on two sampling dates.

GREENHOUSE EXPERIMENT

Description of the Greenhouse

The greenhouse experiment was conducted in the Dalhousie University greenhouse which is equipped with running seawater. Freshwater was supplied by an overhead sprinkler system. Air temperature was maintained above 10°C at all times but was not held at a constant level.

Experimental Design

Eighty-four experimental marsh "plugs" (33 cm diameter x 20 cm depth; 28 per marsh zone) were removed from the Conrods Beach salt marsh and transported to the greenhouse where they were maintained in plastic buckets fitted with flushing ports (Figure 3). Creek edge plugs (microcosms) were flooded with seawater daily and after standing two hours, the seawater was allowed to drain. Midmarsh plugs (microcosms) were flooded daily, and after standing for two hours, were allowed to drain to the sediment surface only, simulating the poorly drained condition of the midmarsh zone. High marsh samples were flooded in the same manner, but for only three consecutive days per two week period, and were allowed to drain completely through top and bottom drainage ports. All microcosms were briefly sprinkled daily with fresh water to simulate rainfall in the field. Laboratory measurements commenced on May 9 (day 1) and the randomly assigned treatments were applied on June 20 (Day 43).

Oil and Dispersant Applications

Oil and dispersant application procedures for the greenhouse study were similar to those used in the field except that the amounts of oil and dispersant were scaled down. The 0.5 mm nominal oil slick was simulated by the application of 400 ml of weathered crude oil per bucket. Four hundred ml of 10% (V:V) dispersant/seawater solution was applied for the dispersant treatments. Microcosms receiving the oil plus dispersant treatment were

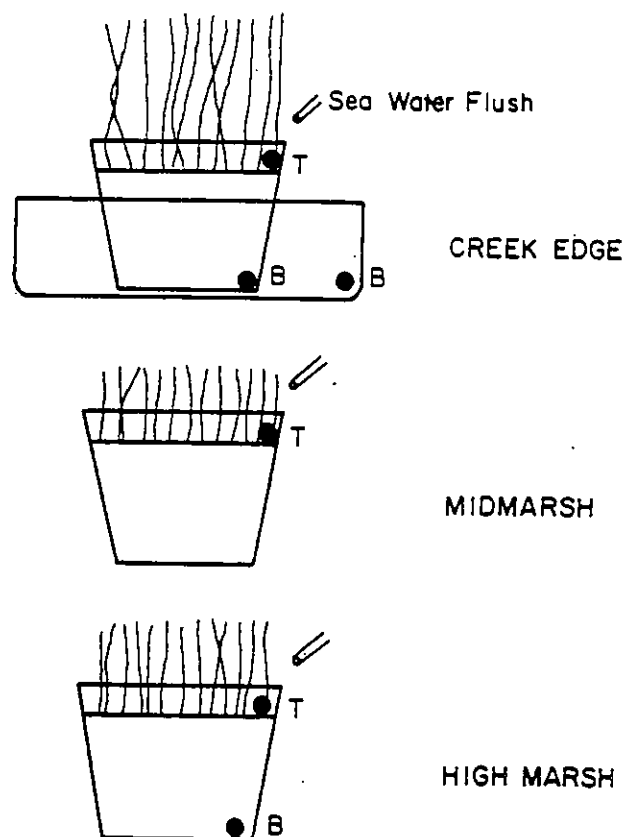


Figure 3: Systems used for maintenance of experimental salt marsh microcosms. Drainage ports are indicated, where present, at top (T) and bottom (B).

first sprayed with 400 ml of weathered crude oil per bucket, followed six hours later by 400 ml of the 10% dispersant/sea water solution.

Physical and Chemical Parameters

Soil Profile - Methods were identical to those used in the field experiment except only one core per treatment was taken on each sampling date.

Soil Chemistry - Analytical techniques for greenhouse soil samples were identical to those used in the field study. The only difference was the number of samples taken per sampling date. In the laboratory study, only one sample was taken for each treatment in each vegetation zone.

Vascular Plant Parameters

Plant Height - In each microcosm, 10 plants were systematically selected at 3 cm intervals along a central transect. These plants were tagged and height measurements were made at regular intervals through the growing season as per the field methods. In instances where a tagged plant had died, its tag was shifted to the nearest plant. Measurements were taken on 9 dates through the growing season. Mean plant height was calculated for each treatment, in each vegetation zone on each sampling date. Growth rates were calculated by subtracting the height of a plant on a particular date by its height on the previous sampling date, then dividing by the number of days elapsed between sampling dates. Growth rates were calculated for the 10 original tagged plants for the interval between each sampling date from day 35 to day 117 for all treatments in all vegetation zones.

Stem Density - A single quadrat was positioned in the center of each microcosm. A 0.01m^{-2} quadrat was used in the creek edge and midmarsh microcosms while a 0.006m^{-2} quadrat was used in the high marsh microcosms. All shoots within each quadrat and all flowering shoots in each microcosm were counted. Measurements were taken on six dates. Mean stem density was calculated for each treatment in each vegetation zone on each sampling date.

Biomass - Biomass harvesting was conducted in the first week of September. All above-ground biomass in each microcosm was harvested, sorted by species, dried at 80°C in a convection oven and weighed on an analytical balance. Detrital biomass was harvested in only the creek edge and high marsh microcosms. All flowering stems in each microcosm were harvested. Reproductive portions were removed from vegetative portions and were weighed separately. Mean above-ground standing crops for living plants,

detritus, and reproductive tissues were calculated for each treatment in each vegetation zone.

Species Cover - The methods followed those for the field experiment except that cover estimates were made on two occasions.

Leaf Anatomy - Methods followed those for the field experiment.

Fluorometry - An explanation of the principle of plant fluorescence induction measurements is presented in Appendix 1. Two plants were randomly selected from each microcosm for fluorometric analysis. Oiled leaves were rejected if selected because of possible interference of light transmission or fluorescence by the oil film. The upper-most fully expanded leaf was removed. All of the leaves from a particular vegetation zone were placed on a pad of water soaked absorbent paper and were covered with a layer of opaque plastic. These leaves were allowed to equilibrate in the dark for one hour. Working illumination was provided by a 6 volt lantern with its lens covered with a green filter. Fluorometric measurements were made with a Brancker Research Ltd. (Ottawa) model SF-20 portable fluorometer. The fluorescence induction curve was monitored for 10 seconds for one of the leaves in each pair while the second was monitored for 100 seconds. Initial, peak, transient peak, and final readings were recorded (Figure 4). Values used in analysis included the peak variable fluorescence (peak reading minus initial reading) and the 100 second difference value (value at 100 seconds minus initial value).

Microbial Population Parameters

Algal and Bacterial Mat Cover - Color characteristics were used to describe the surface distributions of algal mat (green), sulfate-reducing bacteria (black), sulfur-oxidizing bacteria (white) and photosynthetic bacteria (pink). Percent cover values of algal mat, sulfate-reducing bacteria, sulfur oxidizing bacteria and photosynthetic bacteria were estimated in each microcosm. Mean cover values were calculated for all categories for each treatment in the three vegetation zones on two sampling dates.

Heterotrophic Activity - A sample of salt marsh sod (30 cm diameter, 30 cm deep), was taken from the midmarsh vegetation zone. The above-ground vegetation was removed and the sod was cut into pieces such that no root fragments were longer than 2 cm. This material was thoroughly mixed and twenty, 500 g samples were placed in 1 liter mason jars along with 200 ml of seawater. Samples designated for oil or oil plus dispersant treatments had 12.25 ml of Alberta sweet blend crude oil poured onto the surface of the seawater in the mason jars. The seawater was then removed by siphon to

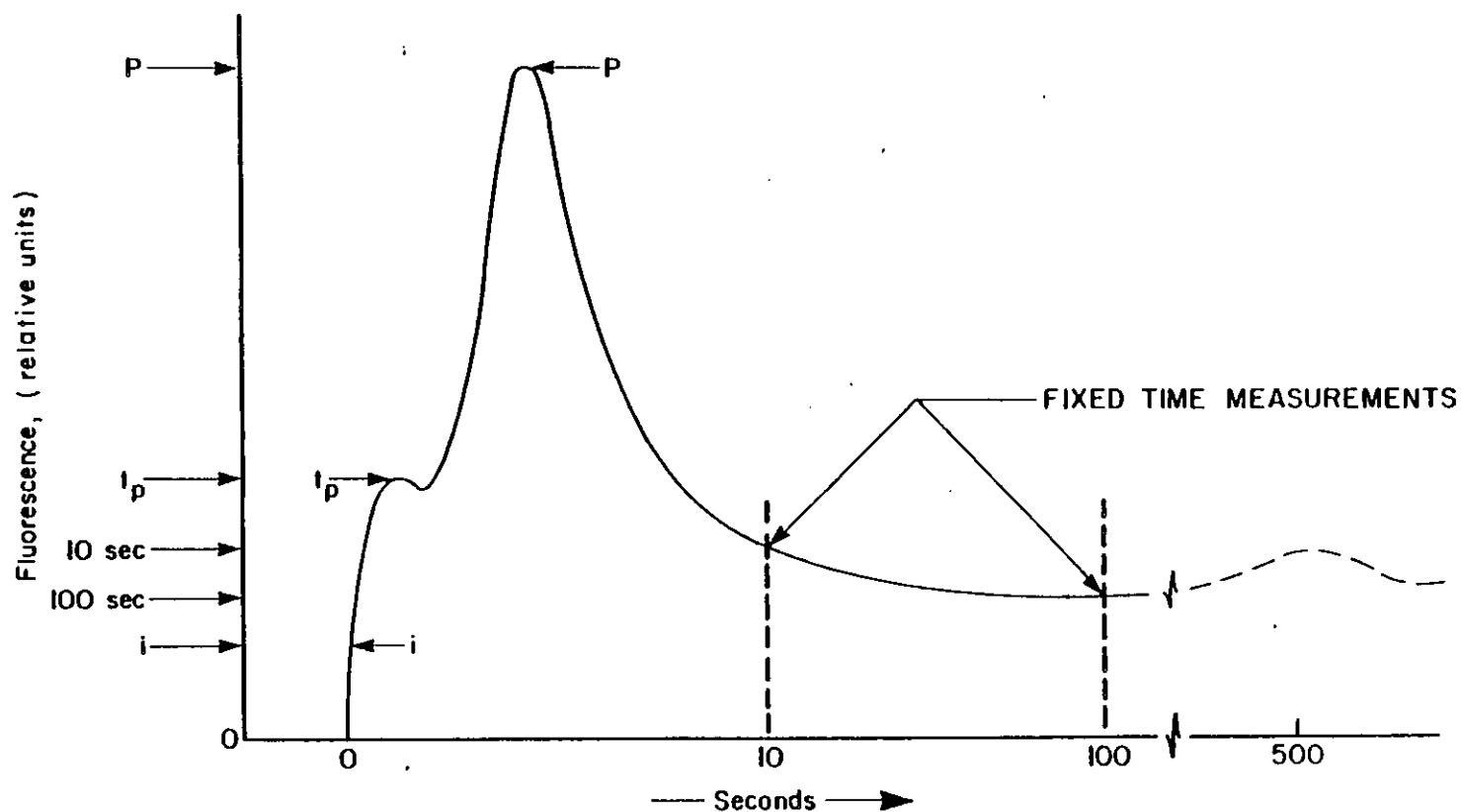


Figure 4: Diagrammatic representation of the fluorescence induction curve for *Spartina alterniflora*. Arrows indicate recorded fluorescence values. The initial value (i) is the "fixed fluorescence". Fluorescence above this level is "variable fluorescence" which is sensitive to the physiological status of the photosynthetic system. We refer to (P-i) as "peak fluorescence" and 100 second values minus initial values as the "100 second difference values". i_p refers to a transient peak.

allow the oil to settle on the sediment surfaces as evenly as possible. Dispersant and oil plus dispersant treated samples had 12.25 ml of a 10% solution of corexit 9527 and seawater sprayed onto the sediment surface with a plant mister. For the oil plus dispersant treatment, the application of the dispersant solution was delayed for 6 hrs to simulate the procedures used in the field. Heterotrophic activity (carbon dioxide production) was monitored 1 and 8 days prior to treatment applications and 4, 7, 13, 26 and 35 days after application. Seawater in the mason jars was replaced each week. Prior to analysis, the bottles were sealed with serum stoppers. Samples were taken at one and four hour intervals and analysed for carbon dioxide using an infra-red gas analyser.

Nitrogenase Activity and Composition of the Mud Surface Microbial Community

- The procedures used for measuring soil surface nitrogenase activity (nitrogen fixation) by the acetylene reduction technique follow those of Patriquin and McClung (1978). Three plugs taken from each bucket with a sharpened cork borer of 0.5 mm diameter to a depth of 1 cm were extruded into a 50 ml flask. Two milliliters of seawater were added, the flasks were closed with serum stoppers, and 3 ml acetylene was added. Flasks were incubated under 6000 lux for 12 hours, and then in the dark for a further 12 hours. Gas samples for analysis of ethylene were taken at 12 and 24 hours. Analysis of nitrogenase activity was conducted on four dates.

Forceps were used to remove a small sample of the sediment surface from a random location in each of the buckets from which cores had been removed. Samples from each vegetation zone were pooled by treatment, thoroughly mixed and then examined under a microscope. The microorganisms observed were classified on the basis of morphological features and the relative abundance of each group (rare, uncommon, common and abundant), was recorded. One sample from each treatment in each zone was examined immediately following each assay of nitrogenase activity.

Statistical Analyses

Statistical analyses were conducted on plant height, stem density, total above-ground biomass, reproductive biomass, Spartina cover, fluorometry, algal cover, heterotrophic activity, and nitrogenase activity data in both the field and greenhouse experiments. Prior to analysis the data were tested for normality and homoscedasticity. In instances where these conditions were not met, the data were transformed. Data analysis was conducted using the SPSS statistical package (Nie et al. 1975).

The first step of data analysis consisted of a preliminary analysis of variance which treated replicate as a factor. This analysis consisted of a

three-way analysis of variance with blocked design and no replication. The purpose of this analysis was to verify that true replication had been achieved by testing whether replicate as a factor had a significant effect.

The second step in the statistical analysis was the use of one-way analysis of variance to test for treatment effects among the four levels of the treatment factor (control, oil, dispersant, and oil plus dispersant). In instances where the means of the four levels were significantly different, the Student-Newman-Keuls multiple range test was used to determine which pairs of treatments were significantly different.

Finally, the data were subjected to a two-way factorial analysis of variance, the results of which indicated which factors (oil and dispersant) had significant effects and whether there was any interaction between these factors.

RESULTS AND DISCUSSION

PHYSICAL AND CHEMICAL EFFECTS

Field Experiment

Tidal Analysis - A comparison of the tidal readings recorded by water level recorder and theodolite are presented in Figure 5. The good correlation between the two methods ($r=0.98$) indicates that the data from both methods are compatible, therefore, comparisons of tidal height and plot elevation are valid.

Figure 6 shows the cumulative frequency distribution of immersion time at different elevations. The mean elevations of the plots in the three vegetation zones are located on the diagram. As shown on Figure 6, test sites located in the creek edge zone were covered with water 10 to 25% of the time. Test sites located in the high marsh zone were covered with water less than 15% of the time, and, in some cases, less than 5% of the time, during the two week period of observations. The midmarsh zone had the narrowest range of elevations and was immersed between 5 and 12% of the time. The results indicated that boundaries between the three vegetation zones were not related to submergence periods, since the three types of vegetation were found at the same levels in the marsh. All sampling sites were located in a narrow band of elevations ranging from 67 to 96 cm above the tide gauge pressure sensor. The highest level of the tide was 113 cm which was 17 cm above the level of the highest quadrat and 46 cm above the lowest quadrat. In both cases, most of the mature vegetation was partly exposed to air during the highest tide.

To illustrate this situation, data from Figure 6 was plotted in Figure 7, with elevation data grouped according to a semi-diurnal tidal cycle. The frequency of submergence periods represent the number of tidal cycles during which the quadrats were submerged instead of the amount of time that they were submerged. According to this figure, the plots were inundated by the rising tide during 35 to 95% of the tidal cycles. If oil was present in the water, those sites would likely be in contact with oil. This confirms that the selection of test sites was representative of an area highly susceptible to oil spills.

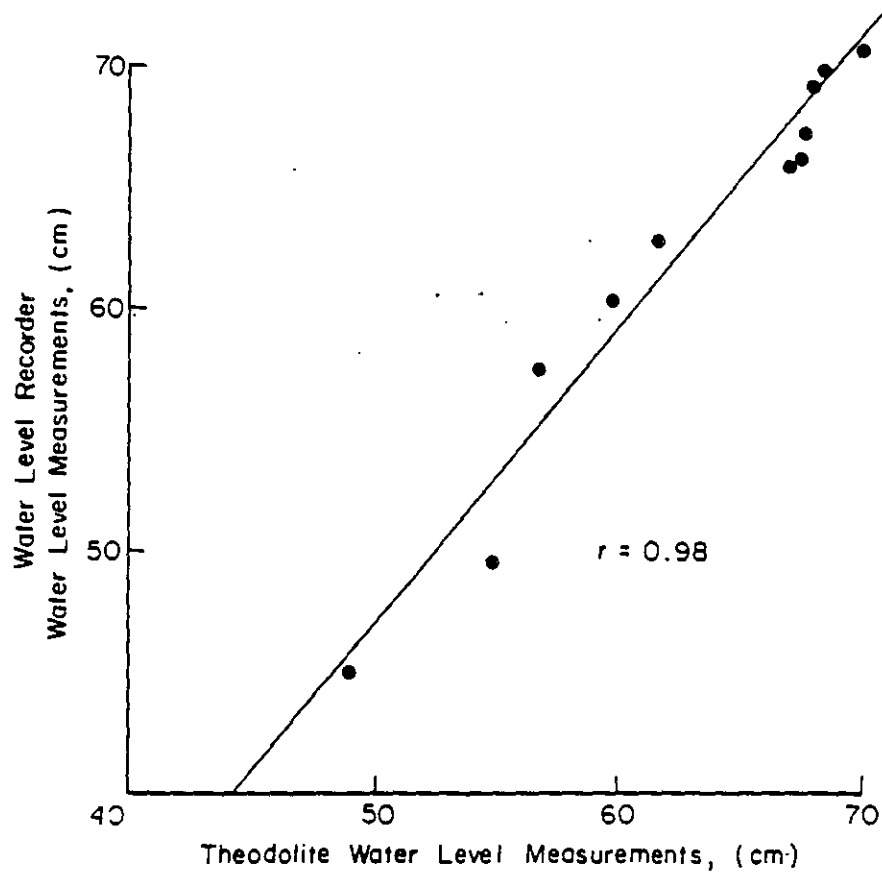


Figure 5: Comparison of theodolite and water level recorder measurements of tide height.

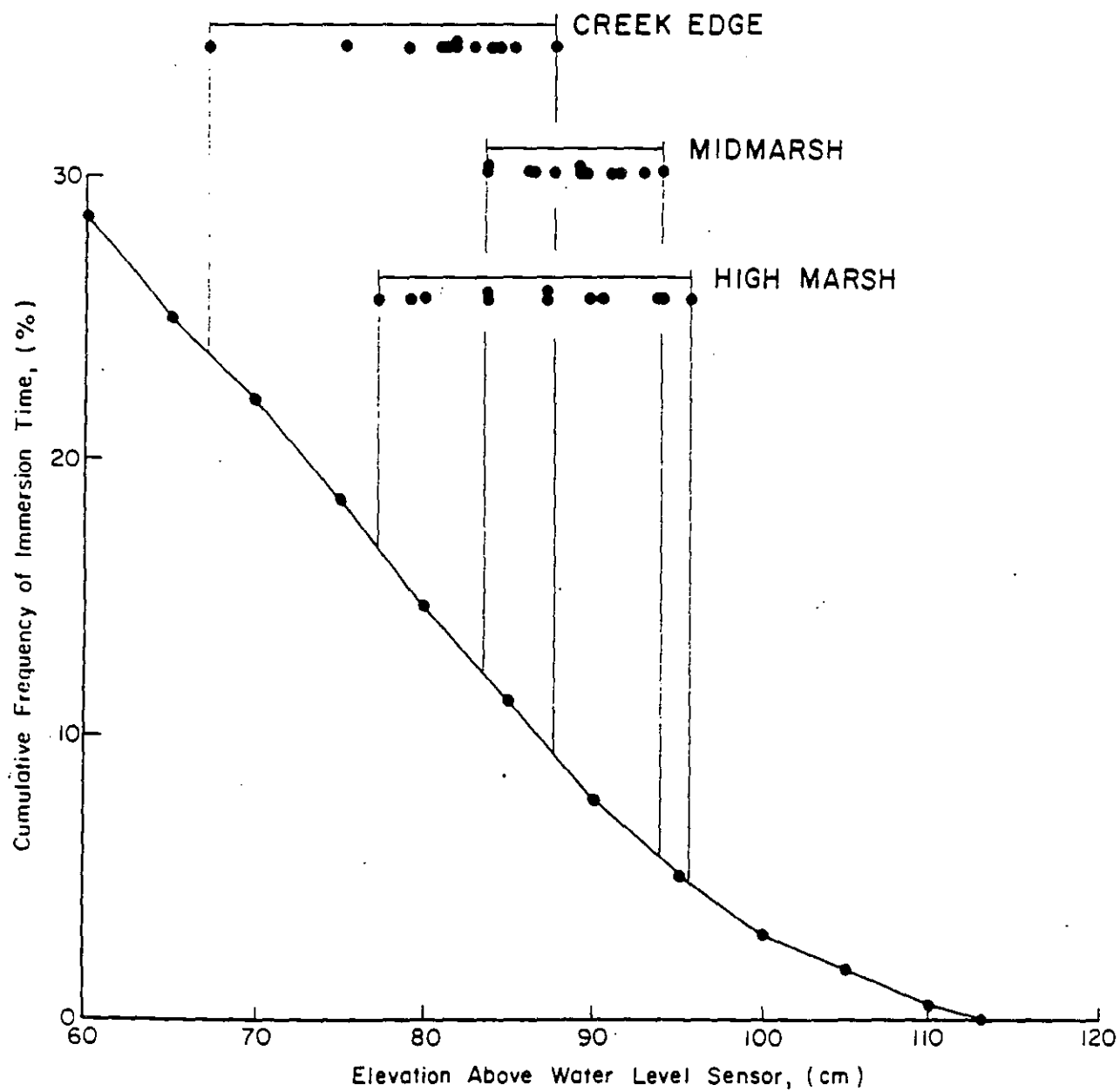


Figure 6: Cumulative frequency of immersion time versus elevation.

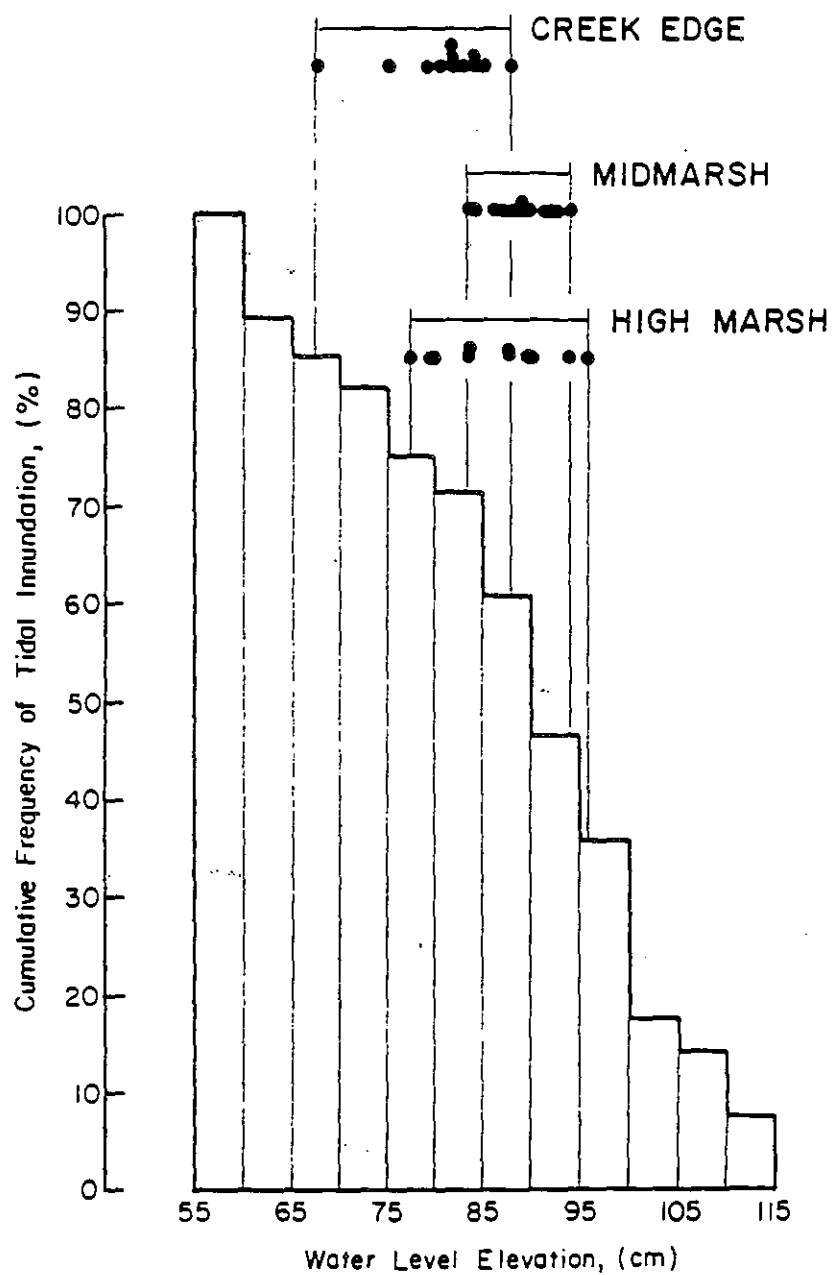


Figure 7: Histogram illustrating the proportion of tidal cycles during which tidal waters reached or surpassed specified elevations.

Soil Profile Descriptions - The oil and/or dispersant treatments could alter the appearance of the sediments by changing the patterns of soil aeration. The death of the vegetation would disrupt plant-mediated soil aeration (via lacunae) and the deposition of a layer of low viscosity weathered oil over the sediment surface could create a barrier to diffusion of oxygen into the sediments. Decreased soil aeration could result in the development or expansion of reducing zones (black in colour) and changes in root coloration (they are black if the roots are not aerating the rhizosphere and brown or white if they are aerating the rhizosphere). Disruption of soil aeration would be expected to be most severe in the waterlogged midmarsh zone where Spartina alterniflora is an important factor in soil aeration. The vegetation of this zone was also the most sensitive to the oil and/or dispersant treatments.

Soil aeration was found to be highly variable in spatial distribution, consequently, temporal changes in soil aeration associated with the treatments were difficult to distinguish. Table 1 presents the thickness of the reducing zones in quadrats receiving the various oil and/or dispersant treatments. Average reducing zone thicknesses in the oil and oil plus dispersant treated quadrats appeared to increase after treatment applications, however, the results were highly variable.

Root coloration did not appear to be affected by the treatments. Living roots ranged from white to light tan in colour regardless of treatment.

Root development differed between vegetation zones and was independent of treatment. The high marsh and midmarsh zones had well developed networks of fine roots, while the root systems of the creek edge zone were characterized by well developed rhizomes but poorly developed mats of fine roots. Aerated microzones were evident in the fine root mats of the midmarsh and high marsh quadrats but were generally absent in the creek edge quadrats where fine roots were relatively scarce. No apparent declines in the extent of these zones were associated with any of the oil and/or dispersant treatments.

Soil Temperature - The oil and/or dispersant treatments had little effect on the soil temperature of the three vegetation zones (Figure 8). For the high marsh and creek edge zones, the relationships of temperatures in the various treatments were similar before and after treatment applications. In the midmarsh zone, oil and/or dispersant treated quadrats were on average 1.1°C (7%) warmer than the control. Albedo may have been decreased by the high mortality of vegetation for all treatments in this zone resulting in a small increase in soil temperature. Soil temperature differences between vegetation zones, however, were larger than soil

Table 1 - Average thickness of reducing zones in oil and/or dispersant treated creek edge, midmarsh, and high marsh field plots on five sampling dates. Treatments were applied on day 60.

Zone	Treatment	Average Reducing Zone Thickness (cm (1 standard deviation))				
		July 9 Day 62	July 24 Day 77	Aug. 6 Day 90	Aug. 21 Day 105	Sept. 15 Day 130
Creek Edge	Control	-	2.0	0.5	0	0
	Oil	0	0.3(0.6)	0.8(1.4)	1.4(2.3)	0
	Dispersant	0	-	0	0	0
	Oil plus Dispersant	0.1(0.1)	2.0(3.5)	1.2(1.3)	2.0(1.7)	1.7(1.8)
Midmarsh	Control	-	0	1.5	2.0	0.5
	Oil	0.6(0.8)	1.8(1.4)	0.7(0.6)	1.7(0.6)	0.8(0.8)
	Dispersant	-	2.0	1.0	4.0	1.0
	Oil plus Dispersant	0.2(0.3)	0.8(1.0)	0.7(0.8)	0.6(0.8)	2.2(2.6)
High Marsh	Control	0	0	0	0	0
	Oil	0.5(0.5)	1.2(0.8)	0	1.8(1.6)	3.0(3.0)
	Dispersant	-	0	0	-	1.0
	Oil plus Dispersant	0	1.2(2.0)	0.3(0.6)	1.5(2.6)	1.7(2.5)

Soil Temperature at 5 cm Below Soil Surface, (C°)

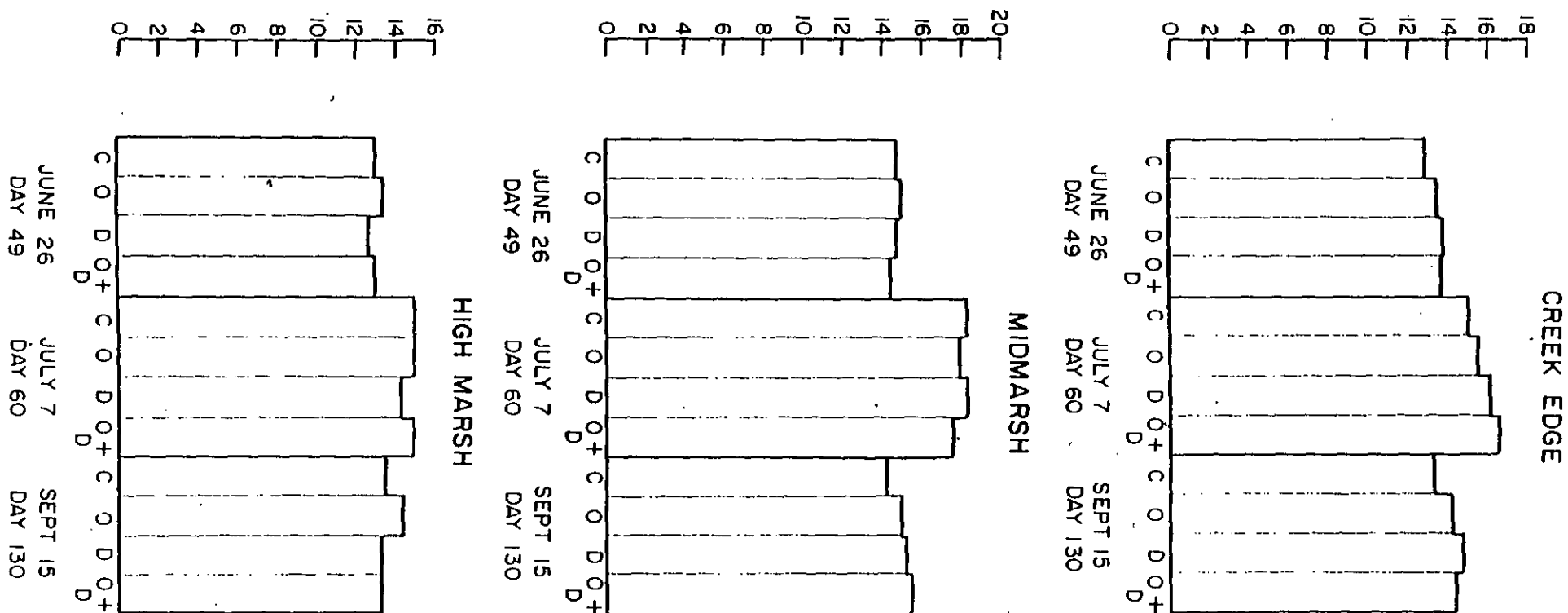


Figure 8: Histogram of mean soil temperatures in the control, oil and/or dispersant treated plots in the creek edge, midmarsh, and high marsh vegetation zones on three sampling dates.

temperature differences between treatments within a particular vegetation zone. The midmarsh zone was consistently warmer than the creek edge and high marsh zones which had similar soil temperatures. This is probably attributable to the relatively low vegetation cover in the midmarsh zone which would reduce the albedo of this zone.

Soil Chemistry - Based on visual observations of both the field and laboratory experiments, the dispersant was not effective in removing oil from the test plots. There are several possible explanations for this: 1) the oil may have been too weathered for the dispersant to penetrate and mix with it, 2) the tidal flushing may not have been energetic enough to disperse the oil, or 3) the dispersant may not have contacted the oil.

The oil, as applied to the test plots, was artificially weathered to about 15% weight loss. Based on results from other studies with this oil (Bobra and Chung, 1986) this translates into a viscosity of 50 cP at 15°C, certainly within the range of viscosity considered to be dispersible. The oil is also known to be easily dispersible in standard laboratory effectiveness tests using Corexit 9527.

Without additional tests (i.e. plots with premixed oil and dispersant), it was impossible to determine whether or not the reason for the apparent ineffectiveness of the dispersant was because of lack of mixing energy or inefficient application. Recent dispersant effectiveness tests in shallow sloughs in Alberta during low winds indicated that dispersants can be effective in dispersing a significant portion of oil on water under very low energy conditions (Quaife et al. 1986). The oil in this study was applied to the plots at low tide and much of the oil was retained by sediment and foliage to the point that it was not floated by the incoming tide but remained submerged. It is thus not surprising that the dispersant was ineffective.

The dispersant/water mixture was applied as a mist from backpack sprayers from above the marsh grass. It is possible that, although this resulted in an even application of dispersant over the test plot (as evidenced by the consistency of the impact data), the dispersant ended up on the leaves rather than the oil. Any coalesced drops of dispersant would tend to run down the stems of the plants to the sediment, thus missing any oil on surrounding surfaces.

In summary, although it is clear in this study that the dispersant was ineffective in removing oil from the saltmarsh test plots, it is not certain whether this means that dispersants are inherently ineffective in saltmarsh environments or that the method of application prevented the

dispersant from reaching and mixing with the oil. Similarly, Baker et al. (1984) noted that the dispersant BP 1100 WD failed to effectively remove oil from salt marsh vegetation. Further experiments, including premixed oil and dispersant, dispersant applied to oil slicks on water in a saltmarsh on an incoming tide and the use of low pressure water flushing to assist dispersion, need to be conducted before the efficacy of dispersant use in saltmarshes can be definitively evaluated.

The oil, as applied to the test plots, had been artificially weathered by exposure to air and sunlight in open containers for 24 h. Weathering was assisted by regular mixing. This resulted in about 15% weight loss. Figure 9 shows the subsequent evaporative loss of the oil on test plots, both in the laboratory and in the field, determined by comparing gas chromatographic (GC) traces of samples with those of a laboratory weathered standard. As shown, the oil evaporated an additional 15% by weight during the first day, after which the evaporation rate was very slow. The scatter of the data points is most likely related to sampling different areas, with different oil thicknesses, in each test plot. The extraction/GC procedure produces an error of about 3% as indicated by the replicate creek edge data points.

Figure 10 shows the results of oil-in-sediment samples taken from both the field and laboratory test plots throughout the study. The data scatter is considerable; this is most likely related to variation in sampling of the sediments. Statistical analysis of replicate samples shows that the average reproducibility of the extraction/ultra-violet (UV) fluorescence analysis is $\pm 20\%$. Conversion of the UV results from ppm chrysene to ppm oil is accurate to within 10%.

The chrysene detection technique (UV fluorescence at 256 nm) would not detect the presence of dispersant, thus no information is available on the penetration or residence time of dispersant in the sediments. The dispersant-only test plot data are shown only as a measure of the repeatability of the extraction/analysis technique, compared to the control plot data.

The scatter in the data is so severe that no conclusions can be drawn regarding differences between plots in different marsh locations, differences between oil alone and oil and dispersant plots or differences between field and laboratory results. All that can be concluded is that those plots that had oil applied to them had significant amounts of oil remaining on and in the sediment at the end of the experiment. Deluane et al. (1984), investigated the concentration of hydrocarbons in a Louisiana salt marsh subjected to oil and oil plus dispersant treatments.

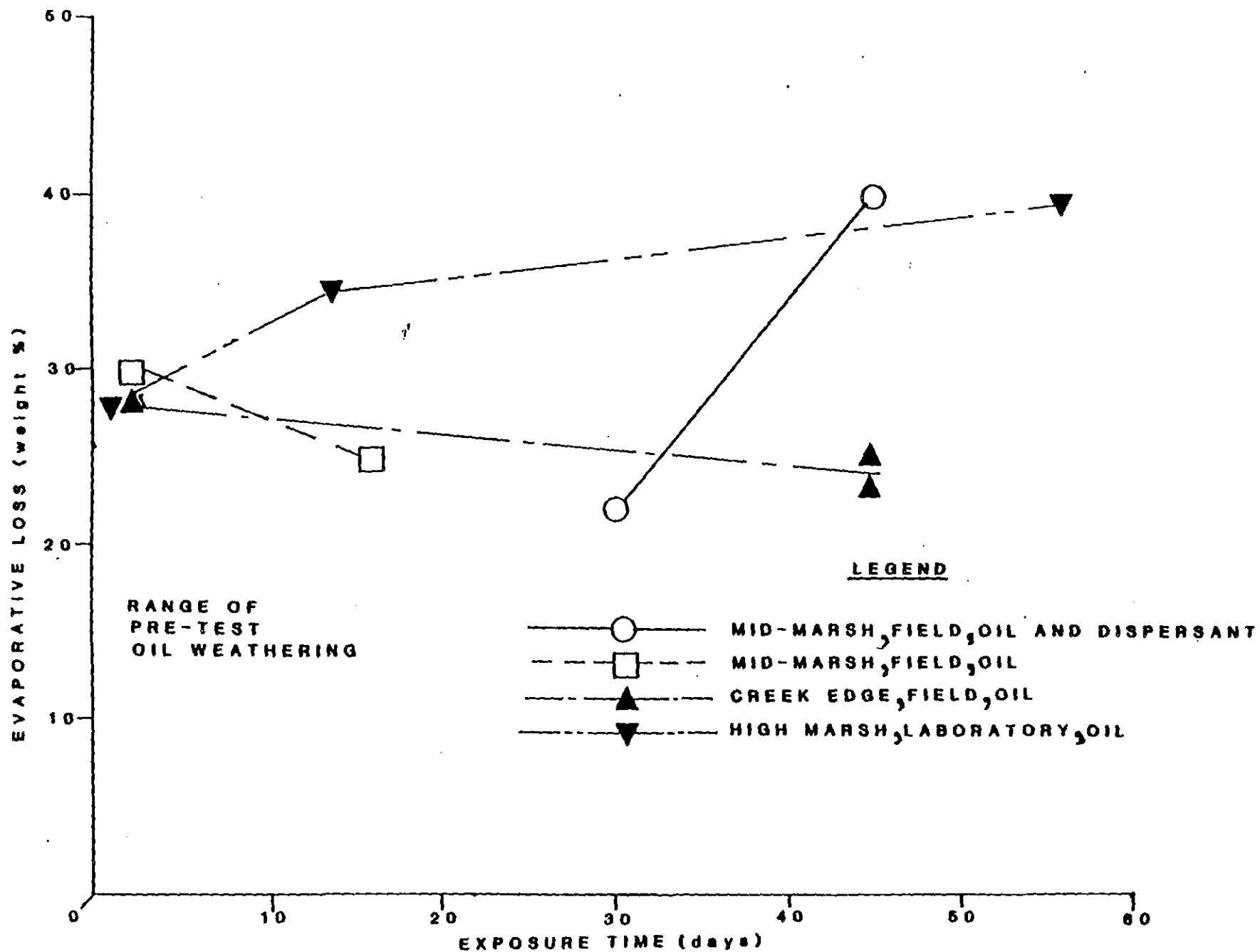


FIGURE 9 - OIL EVAPORATION

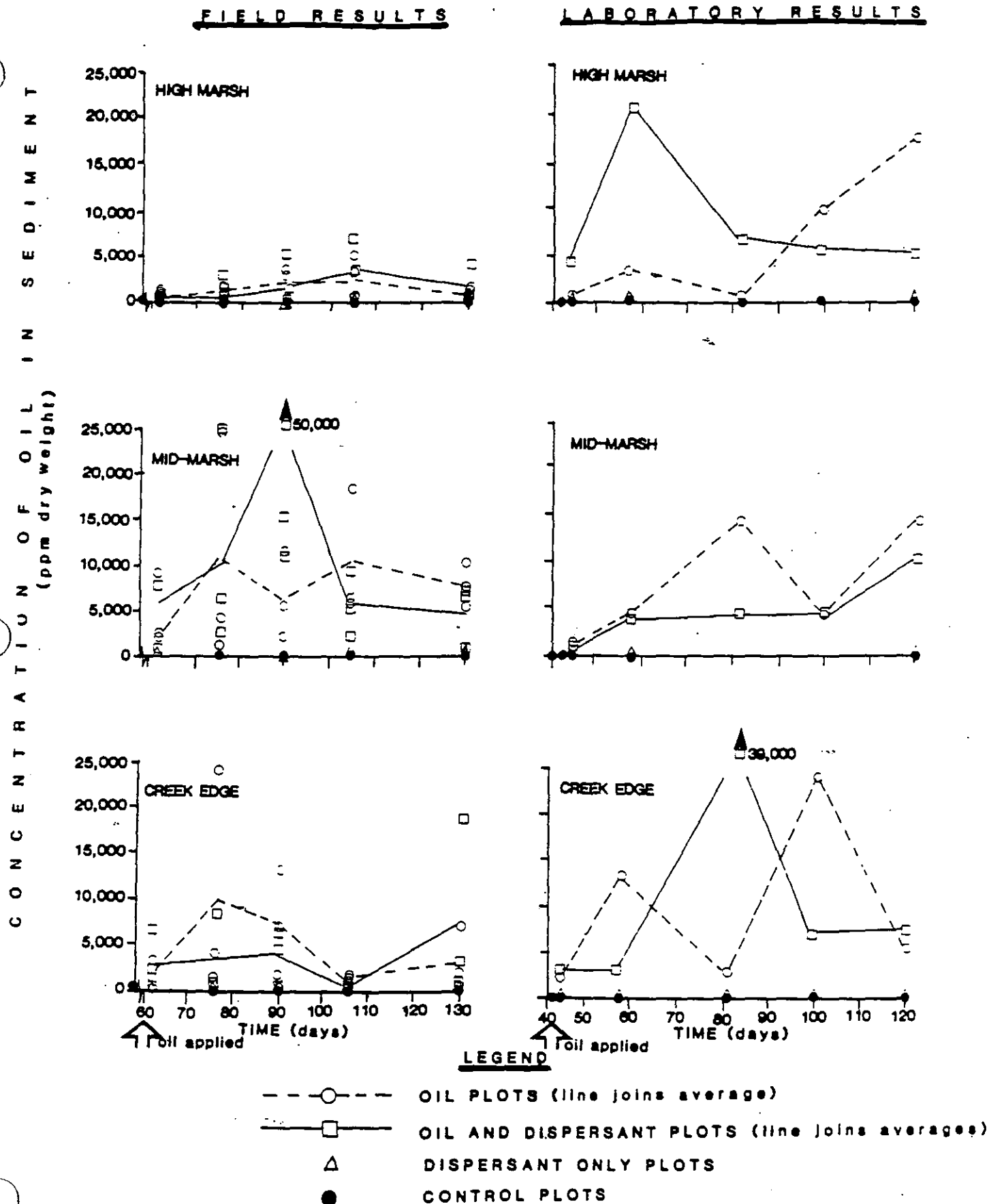


FIGURE 10 - OIL IN SEDIMENT RESULTS

Hydrocarbon concentrations were similar in both treatments. Based on visual observations of the test plots and cores in this study, the oil was contained in the upper few millimetres of sediment and little sediment penetration occurred.

The data suggest that oil-in-sediment concentrations initially increased. Higher concentrations of oil in the sediment may have been caused by the movement of oil from the foliage down the stems to the sediment surface. It also seemed that, in the field, oil-in-sediment concentrations were lower in the high marsh zone than in other areas. Unfortunately, the scatter in the data does not permit a verification of this result.

Greenhouse Experiment

Soil Profile Descriptions - Soil profiles were found to be spatially highly variable in the greenhouse study as in the field study, consequently, temporal changes were difficult to document. Observations revealed a general increase in the volume of anaerobic sediment (as indicated by an increase in grey or black sediment) for the poorly drained midmarsh zone and to a lesser extent the high marsh zone. All treatments (including the control) were similarly affected, suggesting that this was a normal seasonal phenomenon induced by oxygen depletion through heterotrophic activity. Soil profiles for the well drained creek edge zone did not appear to change over time.

Table 2 presents data for the thickness of reducing layers (black) over time for creek edge, midmarsh, and high marsh microcosms receiving the various oil and/or dispersant treatments. Reducing layers were generally restricted to the poorly drained midmarsh zone where they tended to increase through the summer. Similar impacts were noted for all treatments indicating a normal seasonal change.

Extensive colonies of green and pink photosynthetic bacteria were found on the outer surfaces of the marsh plugs. These bacteria were restricted to the midmarsh microcosms because of their preference for anaerobic conditions. All treatments (including the control) had extensive colonies of these bacteria which remained relatively stable through the summer, suggesting that the oil and/or dispersant treatments had no long term effect on them.

Root coloration was not affected by any of the treatments. Root morphology was found to vary between vegetation zones but not between treatments. The midmarsh and high marsh microcosms had well developed fine root systems, while, the creek edge microcosms had poorly developed fine root systems but

Table 2 - Thickness of reducing zones in oil and/or dispersant treated creek edge, midmarsh, and high marsh greenhouse microcosms on four sampling dates. Treatments were applied on day 43.

Zone	Treatment	Reducing Zone Thickness			
		June 21 Day 44	July 28 Day 81	Aug. 15 Day 99	Sept. 5 Day 120
Creek Edge	Control	5.0	0	0	0
	Oil	0	0	0	0
	Dispersant	0	0	0	0
	Oil+Dispersant	0	0	0	0
Midmarsh	Control	0	0	0	3.0
	Oil	0	0	2.0	0
	Dispersant	0	0	1.5	1.0
	Oil+Dispersant	0	1	0	2.5
High Marsh	Control	0	0	0	0
	Oil	0	0	0	0
	Dispersant	0	0	0	0
	Oil+Dispersant	0	0	0	0

large rhizomes. Aerated microzones were common around the fine roots of the midmarsh and high marsh microcosms. No apparent decline in the extent of these zones was noted after treatment application. Aerated microzones were generally not observed in the creek edge microcosms because of the scarcity of fine roots.

Comparison of Physical and Chemical Effects Noted in the Field and Greenhouse Experiments

Although soil profile data were highly variable, the general trends noted in the field and greenhouse studies were very similar. Sediment chemistry data were also highly variable in both experiments. No differences in the retention of oil in the sediments or the effectiveness of the dispersant could be discovered in either the field or greenhouse experiments.

IMPACTS ON VASCULAR PLANTS

Field Experiment

General Observation - Logistical constraints prevented the production of frequent and detailed observations of symptoms in the field experiment. Available observations revealed that the symptoms of oil and/or dispersant toxicity and the rates at which these symptoms progressed were very similar to those observed in the greenhouse experiment. These observations can be found in the general observations section for the greenhouse experiment.

Plant Height - The null hypotheses tested in this experiment were:

- H₁: Oil has no effect on Spartina alterniflora or S. patens height growth.
- H₂: The Dispersant has no effect on Spartina alterniflora or S. patens height growth.
- H₃: There is no interaction (synergistic or inhibitory) between the factors oil and dispersant, affecting height growth of Spartina alterniflora or S. patens.

These hypotheses were tested in each of the three vegetation zones. Results of the Student-Newman-Keuls multiple range tests and two-way analysis of variance procedures used to test these hypotheses are presented in Appendix 2.

In the creek edge zone, plant heights did not differ significantly between plots before treatment applications (Figure 11, Appendix 2). Fifteen days after treatment applications (day 75), plants in the dispersant and oil

CREEK EDGE (Field)

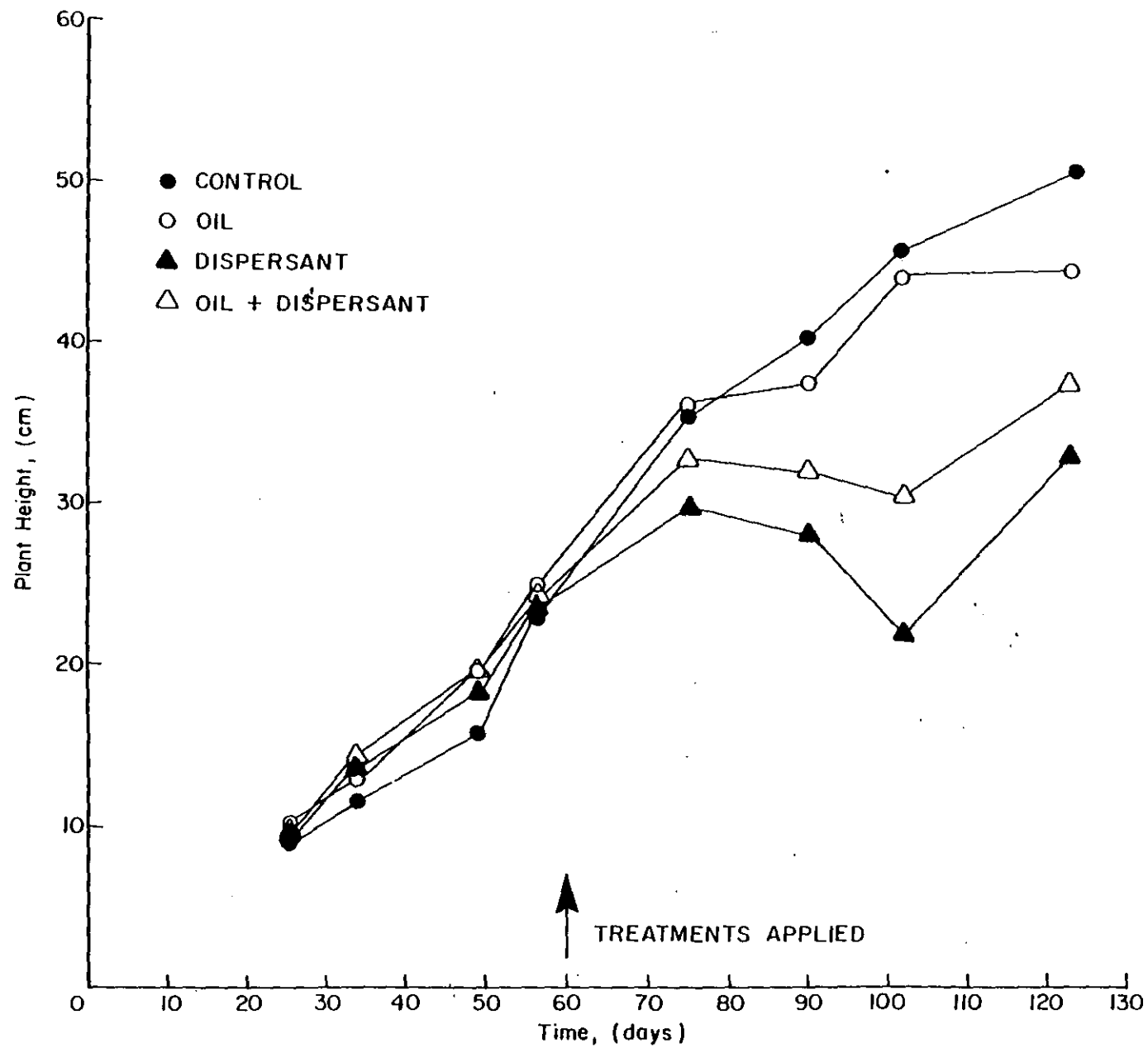


Figure 11: Mean Spartina shoot height for the creek edge zone versus time. Data are from the field study

plus dispersant treated plots were significantly shorter than control plants ($\alpha = 0.05$, $df=2$) and remained significantly shorter than controls for the duration of the experiment. The dispersant treatment caused larger reductions in mean height than the oil plus dispersant treatment. The oil treatment had relatively little long term effect on plant height, although 29 and 63 days after treatment (days 89 and 123) oiled plants were significantly shorter than the controls ($\alpha = 0.05$, $df=2$). These results suggest that the null hypotheses H_1 and H_2 were not true. Plant height in the dispersant and oil plus dispersant treated plots appeared to recover between 42 and 63 days after treatment (days 102 and 123). This trend was probably caused by selective mortality of short plants which would increase mean plant height although growth rates of surviving plants probably did not increase. Short plants would be at a disadvantage in competition for light and nutrients and would therefore suffer more stress than the dominant plants. Higher mortality rates would be expected for smaller plants.

Two way analyses of variance indicated a significant reduction ($\alpha = 0.01$ on days 89 and 102, $\alpha = 0.05$ on day 123, $df=2$) in toxicity (as indicated by reduced plant height) when oil and dispersant were applied together. This inhibitory interaction was noted on all sampling dates following treatment application indicating that hypothesis H_3 was not true. The oil coating on the plants may have neutralized dispersant contacting the plant or it may have plugged or induced closure of the stomata preventing the more toxic dispersant from entering the plant.

In the midmarsh zone, no significant differences ($\alpha = 0.05$, $df=2$) in pretreatment plant heights were noted (Figure 12, Appendix 2). Significant reductions ($\alpha = 0.05$, $df=2$) in plant height were not noted in the zone until 29 days after treatment (day 89). This was unexpected since heavy mortality and expression of severe stress symptoms were noted in all oil and/or dispersant treatments within one week of treatment. Rapid reduction in stem density of small plants may have maintained mean plant height at an artificially high level during this period. Dispersant treated plants were significantly shorter ($\alpha = 0.05$, $df=2$) than controls from 29 days after treatment (day 89) until the end of the experiment. This treatment was associated with the greatest reductions in stem density. The oil plus dispersant treated plants were significantly shorter ($\alpha = 0.05$, $df=2$) than control plants 29 and 42 days after treatment (days 89 and 102) but were not significantly different from the controls 63 days after the treatment (day 123). Stunting associated with this treatment was significantly less ($\alpha = 0.05$, $df=2$) than that caused by the dispersant. Mean plant heights in the dispersant and oil plus dispersant treated plots appeared to recover on the last sampling date, however, this was probably the result of high

MIDMAR (Field)

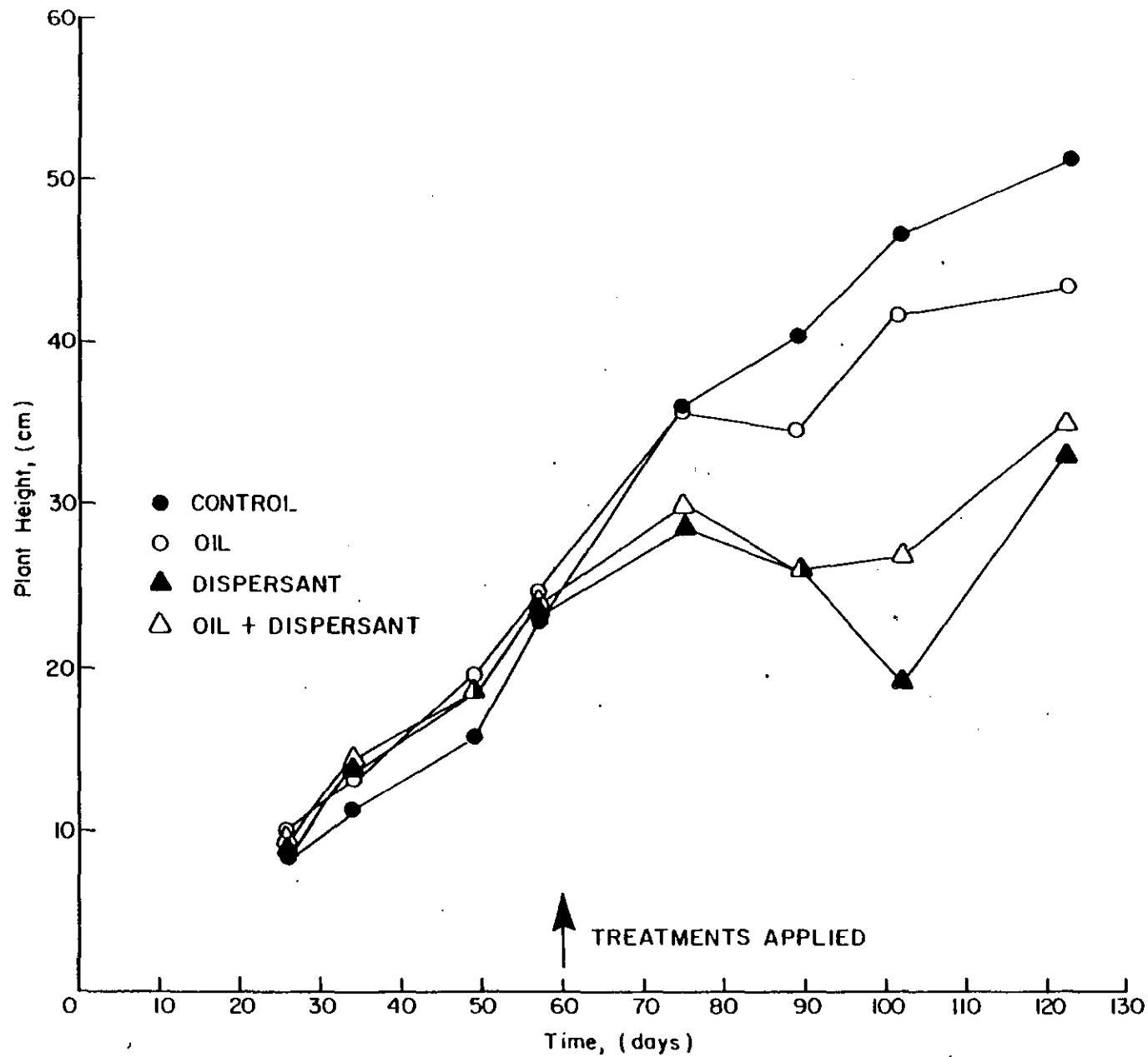


Figure 12: Mean Spartina shoot height for the midmarsh zone versus time. Data are from the field study.

mortality of small plants. Oil treated plants were significantly taller ($\alpha = 0.05$, $df=2$) than either dispersant or oil plus dispersant treated plants, but were significantly shorter ($\alpha = 0.05$, $df=2$) than control plants on days 89 and 123 (29 and 63 days after treatment). These results suggest that the null hypotheses H_1 and H_2 were not true.

Two way analysis of variance results indicated significant reductions in toxicity when oil and dispersant were combined. Significant inhibitory interactions between treatments were noted on days 75 ($\alpha = 0.05$, $df=2$) and 102 ($\alpha = 0.05$, $df=2$) (15 and 42 days after treatment) but not on days 89 and 123 (29 and 63 days after treatment). The oil coating on oil plus dispersant treated plants may have neutralized the dispersant or prevented its entry into the interstitial spaces of the plants via the stomata. Based on these results, the null hypothesis H_3 was rejected.

In the high marsh plots, pretreatment plant heights were not significantly different ($\alpha = 0.05$, $df=2$) on days 26, 34, and 49, however, on day 57 plants in the quadrats slated for oil treatment were significantly shorter ($\alpha = 0.05$, $df=2$) than the controls (Figure 13, Appendix 2). Mean height values were significantly reduced ($\alpha = 0.05$, $df=2$) by the oil and dispersant treatments on all posttreatment sampling dates and both treatments caused similar reductions in shoot height. Mean plant heights for the oil plus dispersant treated plots were similar to those of the dispersant and oil treated plants on days 75 and 89 (15 and 29 days after treatment), however, by day 102 mean plant heights for the oil plus dispersant treated plants had risen to levels similar to the control where it remained until the end of the experiment. This sudden increase was probably caused by high mortality of small plants.

Two way analysis of variance results indicated a significant inhibitory interactions between oil and dispersant toxicity ($\alpha = 0.05$, $df=2$) on all post treatment sampling dates. Neutralization of the dispersant or prevention of its entry into the plants as outlined in the creek edge and high marsh zones probably account for this inhibitory interaction. Based on these results, the null hypothesis H_3 was rejected.

In summary, these data suggest that the midmarsh zone was the most sensitive to the oil and/or dispersant treatments while the high marsh zone was the least sensitive of the three zones. The dispersant treatment was associated with the largest reductions in mean plant height in all vegetation zones, while the oil treatment generally had the least impact.

HIGH MARSH (Field)

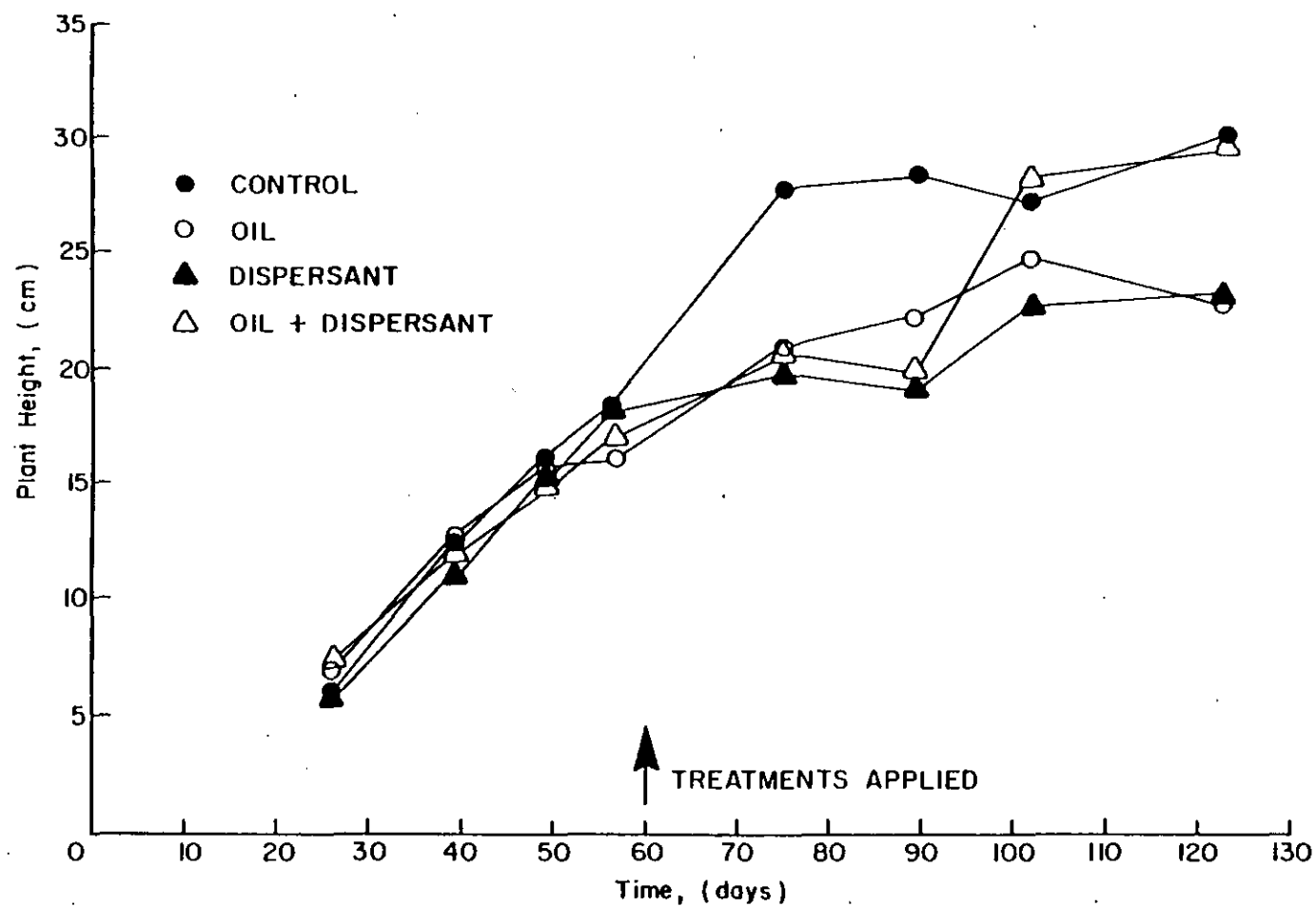


Figure 13: Mean Spartina shoot height for the high marsh zone versus time. Data are from the field study.

Dispersant toxicity was significantly inhibited by the oil in all vegetation zones. Large increases in mean height associated with the dispersant and oil plus dispersant treatments appear to be caused by high mortality of small plants and do not indicate a recovery of growth rates.

Stem Density - The null hypotheses tested in this experiment were:

- H₁: Oil has no effect on Spartina alterniflora or S. patens stem density.
- H₂: The Dispersant has no effect on Spartina alterniflora or S. patens stem density.
- H₃: There is no interaction (synergistic or inhibitory) between the factors oil and dispersant, affecting Spartina alterniflora or S. patens stem density.

These hypotheses were tested in each of the three vegetation zones. Results of the Student-Newman-Keuls multiple range tests and two-way analysis of variance procedures used to test these hypotheses are summarized in Appendix 3.

Stem density in the field varied substantially between plots, making evaluation of trends difficult in some instances. In the creek edge zone, pretreatment stem densities for the plots assigned to the dispersant treatments were significantly lower ($\alpha = 0.05$, $df=2$) than all other treatments on both day 26 and 49 (Figure 14, Appendix 3). On days 89 and 123 (19 and 63 days after treatment), all oil and/or dispersant treatments had significantly lower stem densities ($\alpha = 0.05$, $df=2$) than the control with oil plus dispersant being the most toxic treatment, therefore the null hypotheses H₁ and H₂ were rejected. The stem density trend over time for the dispersant treated quadrats, however, was almost identical to that of the control, suggesting that stem densities may not have been affected by the dispersant treatment. Two-way analysis of variance results indicated a significant synergistic enhancement of toxicity when oil and dispersant were combined. On the basis of these data hypothesis H₃ was rejected.

Significant differences between pretreatment stem density values ($\alpha = 0.05$, $df=2$) were also encountered in the midmarsh zone (Figure 15, Appendix 3). On day 26, there were no significant differences between treatments, however, on day 49, plots assigned to the dispersant and oil plus dispersant treatments had significantly lower ($\alpha = 0.05$, $df=2$) stem densities than the control. Stem densities in all oil and/or dispersant treated plots declined rapidly after treatment application. Oil plus dispersant was the most toxic treatment reducing stem density by over 99% within 29 days of treatment. Stem densities in all oil and/or dispersant

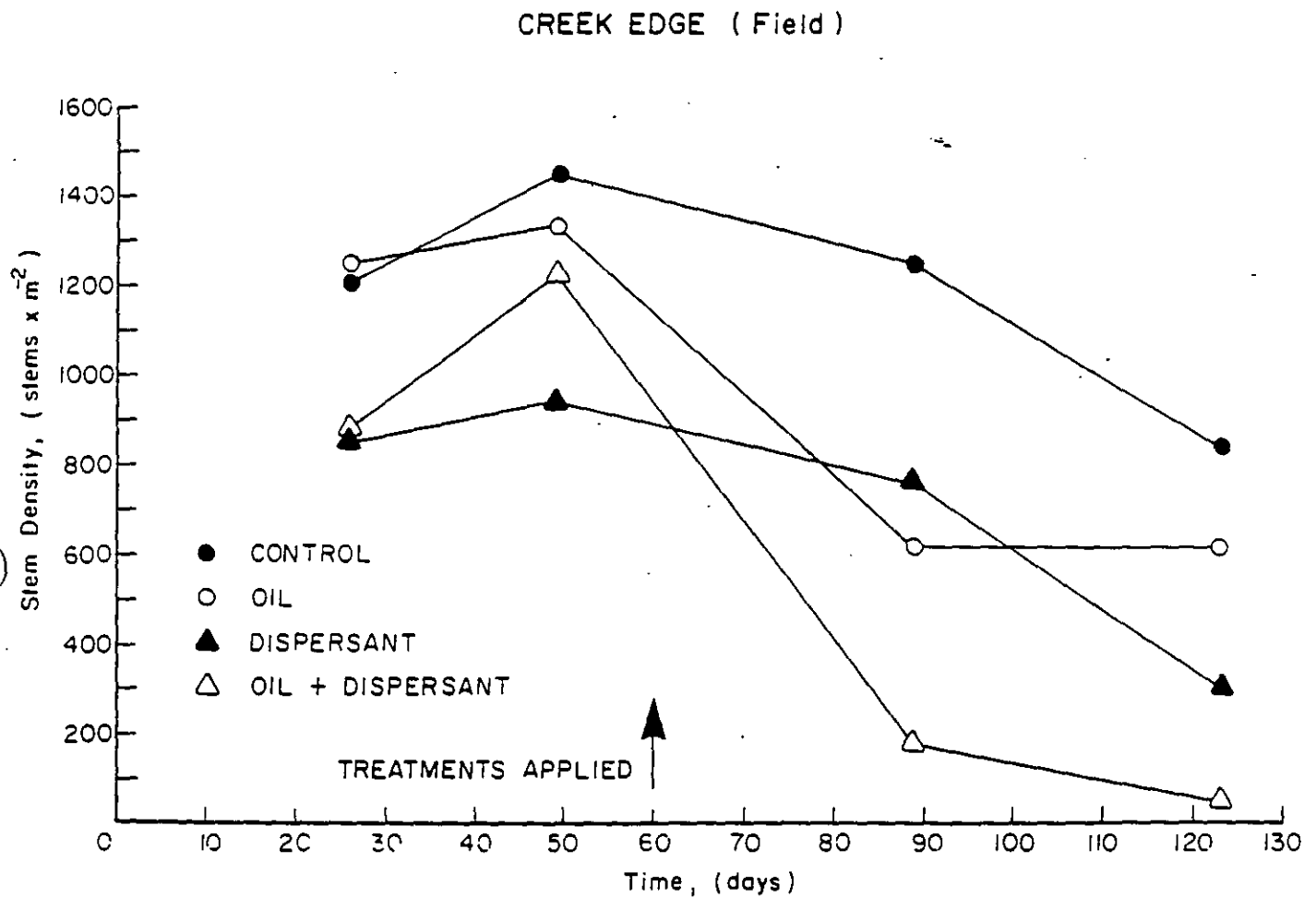


Figure 14: Mean stem density of *Spartina alterniflora* versus time. Data for control, oil and/or dispersant treated field plots from the creek edge zone are presented. Treatments were applied on day 60.

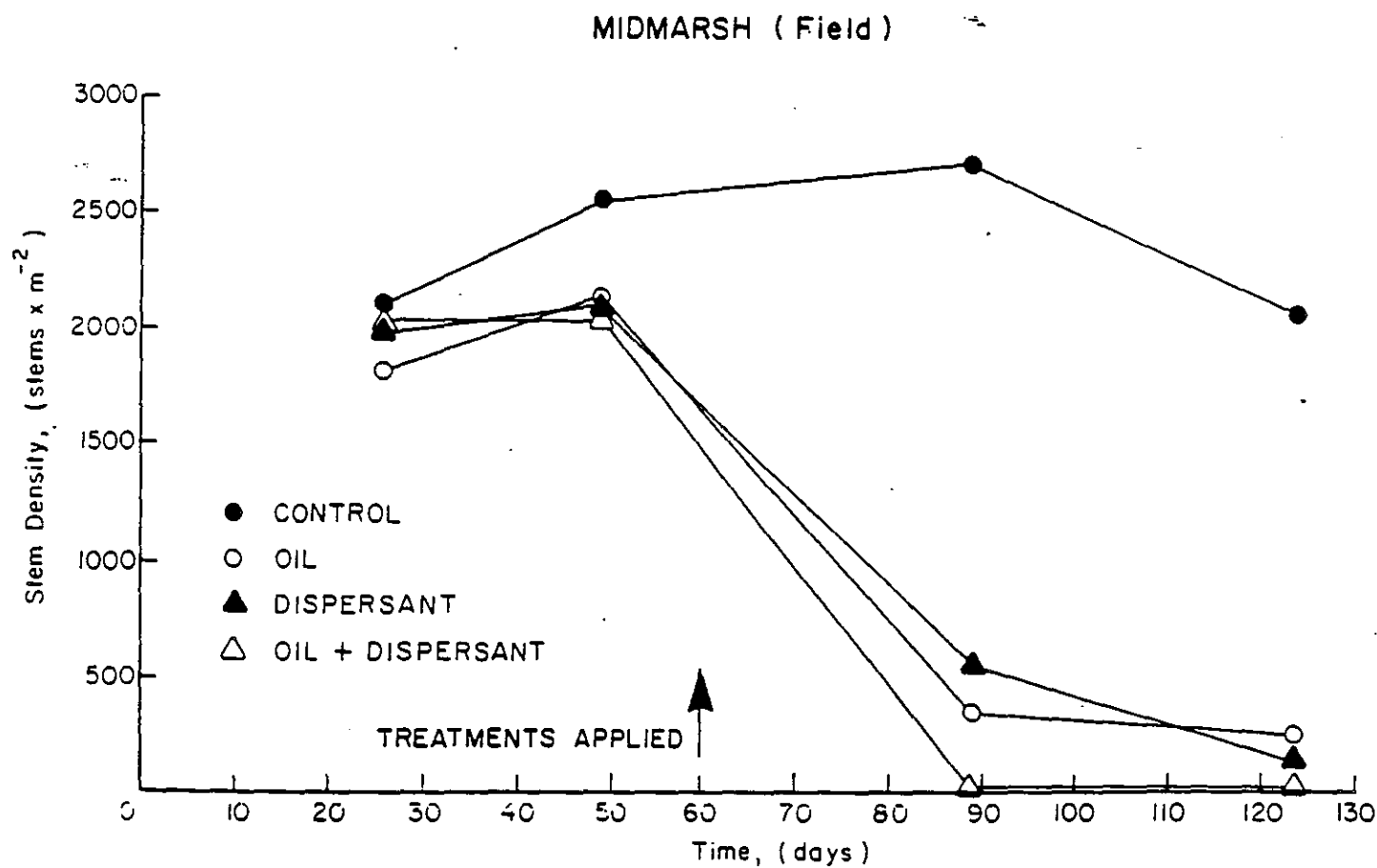


Figure 15: Mean stem density of Spartina alterniflora versus time. Data for control, oil and/or dispersant treated field plots from the midmarsh zone are presented. Treatments were applied on day 60.

treatments were significantly reduced ($\alpha = 0.05$, $df=2$) on all sampling dates following treatment applications, therefore, the null hypotheses H_1 and H_2 were rejected. Two-way analysis of variance indicated a synergistic enhancement of toxicity when oil and dispersant were combined. This interaction was significant ($\alpha = 0.05$, $df=2$ on day 89; $\alpha = 0.01$, $df=2$ on day 123) on all sampling dates following treatment, consequently, H_3 was rejected.

In the high marsh zone (Figure 16, Appendix 3), plots assigned to the oil treatments had stem densities significantly higher ($\alpha = 0.05$, $df=2$) than other plots on day 26, however, by day 49 this difference had disappeared. The application of oil and/or dispersant significantly reduced ($\alpha = 0.05$, $df=2$) stem density for the rest of the growing season, indicating that the null hypotheses H_1 and H_2 were not true. The oil treatment caused the largest reductions. Dispersant and oil plus dispersant induced similar reductions in stem density. Two-way analysis of variance indicated that oil toxicity was significantly inhibited ($\alpha = 0.01$, $df=2$) by the dispersant for all sampling dates following treatment application. The oil appeared to be more toxic to Spartina patens than S. alterniflora, while the dispersant was less toxic to S. patens than S. alterniflora. The dispersant application may have been successful in removing some of the oil from the plants, thereby reducing oil toxicity. Based on these results, the null hypothesis H_3 was rejected. A slight recovery in stem density was noted for all oil and/or dispersant treatments 63 days after treatment (day 123).

In summary, the midmarsh zone was the most sensitive plant community with the oil and/or dispersant treatments causing heavy mortality. The high marsh zone was least sensitive zone with the creek edge zone intermediate in sensitivity.

The various treatments differed in relative toxicity between vegetation zones. Vegetation zones dominated by Spartina alterniflora (creek edge and midmarsh) were impacted the most by the oil plus dispersant. Oil and dispersant treatments had similar effects. There was a significant synergistic enhancement of toxicity when oil and dispersant were applied together. The S. patens dominated zone (high marsh) was affected most by the oil treatment. Dispersant and oil plus dispersant treatments caused similar reductions in stem density. The dispersant significantly inhibited the toxicity of oil in this zone.

Flowering shoot density was severely affected in all zones by all treatments (Table 3). In the midmarsh and creek edge quadrats, flowering shoot density was reduced the most by the oil plus dispersant and least by

HIGH MARSH (Field)

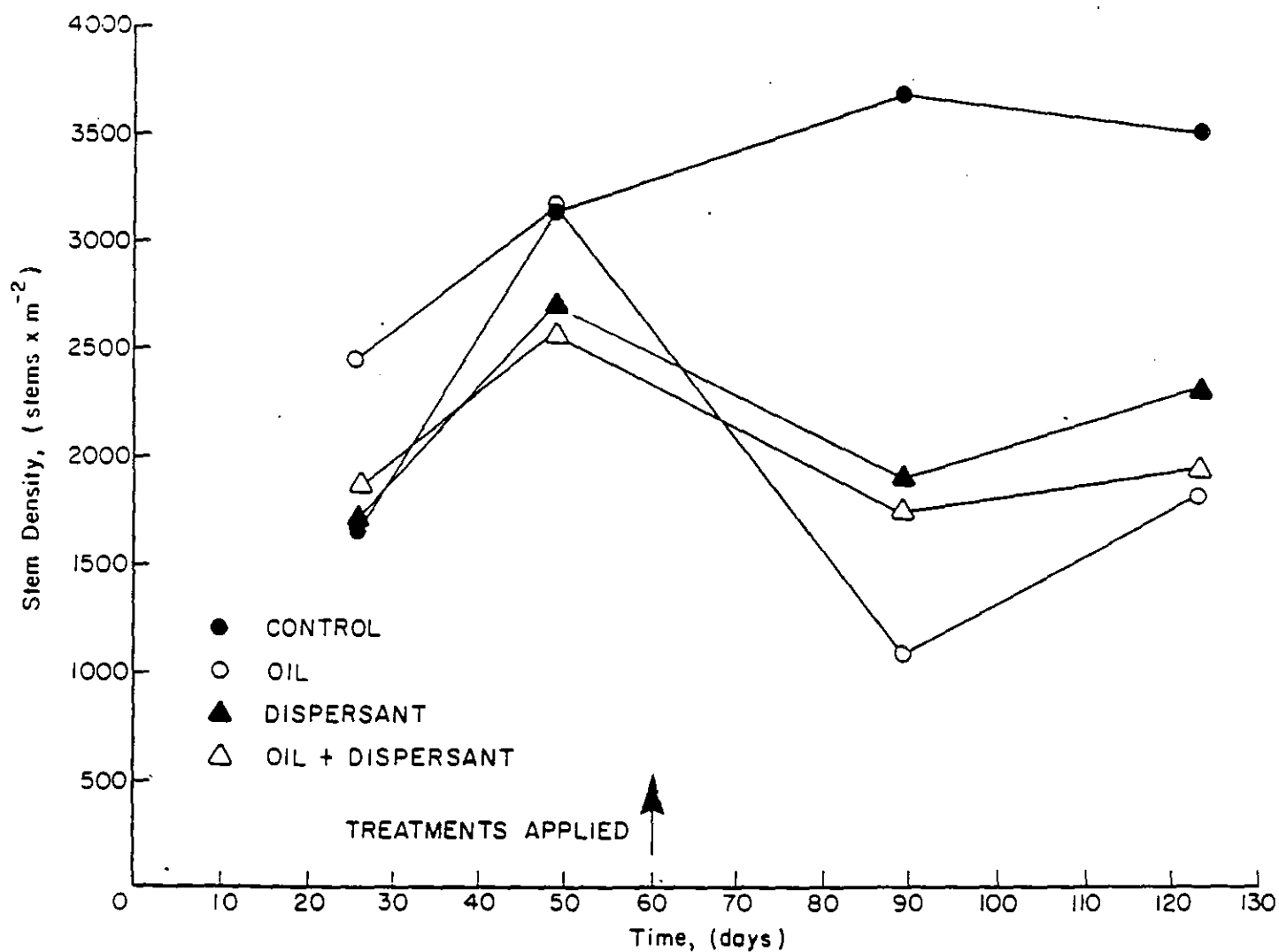


Figure 16: Mean stem density of *Spartina alterniflora* versus time. Data for control, oil and/or dispersant treated field plots from the high marsh zone are presented. Treatments were applied on day 60.

Table 4 - Reproductive biomass (g/m^2) in field plots 119 days after application of oil and/or dispersant to salt marsh vegetation.

	Control	Oil	Dispersant	Oil plus Dispersant
Reproductive Biomass				
Creek Edge	2.884(4.804)	0.033(0.056)	0.134(0.231)	0
Mid Marsh	0.095(0.076)	0.007(0.023)	0	0
Upper Marsh	1.244(1.232)	0.306(0.142)	0	0.002(0.003)

the dispersant. In the midmarsh and high marsh zones, both dispersant and oil plus dispersant had the most impact while oil had the least impact.

Biomass - The null hypotheses tested in this experiment were:

- H₁: Oil has no effect on total above-ground biomass production.
- H₂: Dispersant has no effect on total above-ground biomass production.
- H₃: There is no interaction (synergistic or inhibitory) between the factors oil and dispersant, affecting total above-ground biomass production.

These hypotheses were tested in each of the three vegetation zones. Results of the Student-Newman-Keuls multiple range tests and two-way analysis of variance procedures used to test these hypotheses are summarized in Appendix 4.

Results of the above-ground biomass harvest indicated that the midmarsh zone was very sensitive to all of the treatments. All oil and/or dispersant treatments significantly reduced ($\alpha = 0.05$, $df=2$) above-ground biomass in this zone (Figure 17, Appendix 4). The creek edge zone was intermediate in its sensitivity to the treatments. The dispersant and oil plus dispersant treatments caused significant reductions ($\alpha = 0.05$, $df=2$) in above-ground biomass while the above-ground biomass of oil treated plots was not significantly different from the control quadrats. The high marsh zone was the least sensitive to the treatments. In this zone, only the dispersant treated plots had significantly reduced ($\alpha = 0.05$, $df=2$) above-ground biomass. Based on these results, the null hypothesis H₁ was retained in the creek edge and high marsh zones but was rejected in the midmarsh zone. The null hypothesis H₂ was rejected in all of the vegetation zones. No significant interactions between oil and dispersant were noted in any of the vegetation zones, therefore, the null hypothesis H₃ was retained in all three vegetation zones.

Reproductive biomass was greatly reduced by all oil and/or dispersant treatments (Table 4, Appendix 4). The distribution of reproductive plants was very patchy, making the data highly variable, consequently, the reductions in reproductive biomass were not significant.

Species Cover - The null hypotheses tested in this experiment were:

- H₁: Oil has no effect on Spartina alterniflora or S. patens cover.
- H₂: Dispersant has no effect on Spartina alterniflora or S. patens cover.

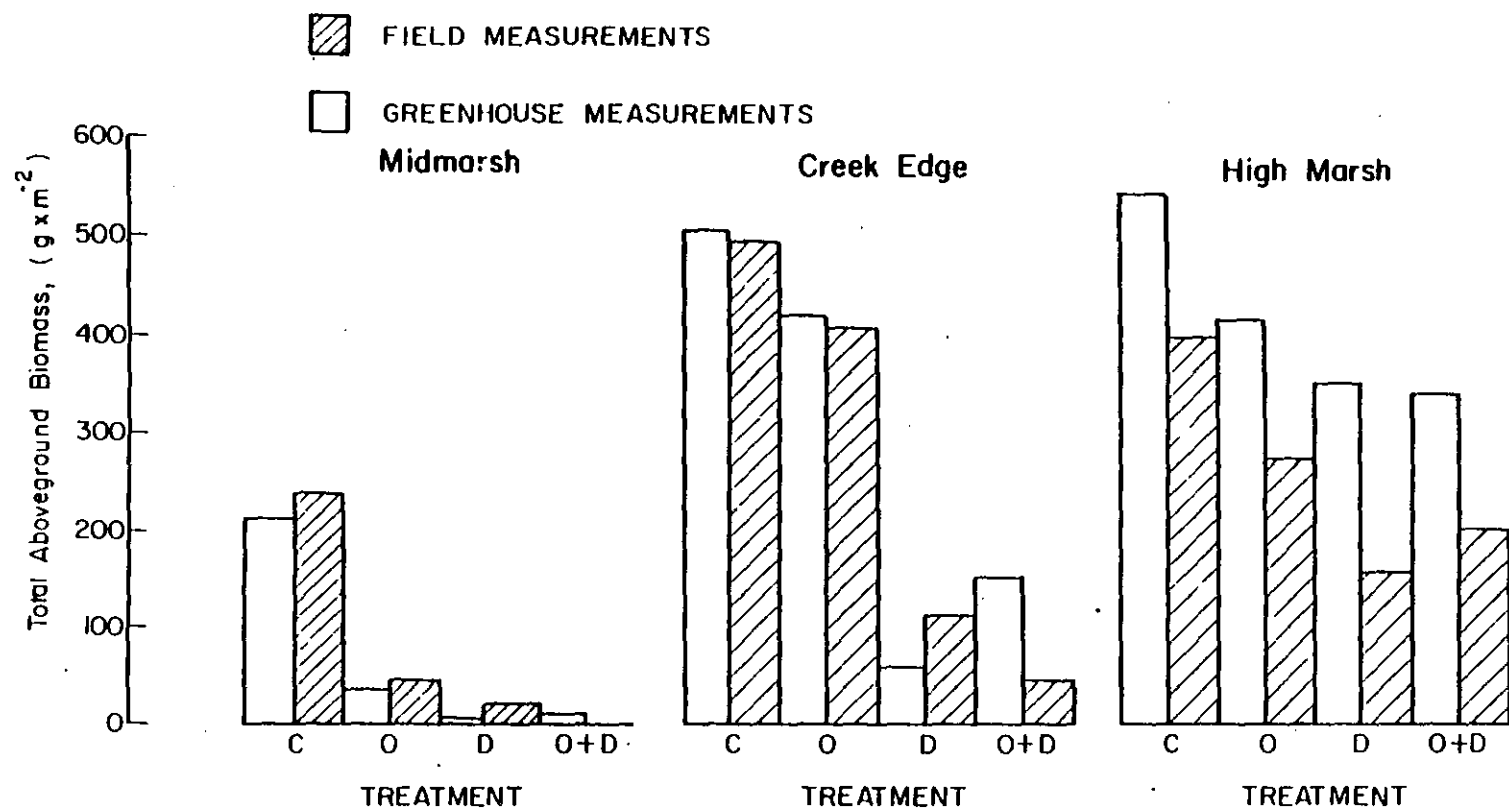


Figure 17: Biomass harvested from salt marsh plots and microcosms 119 days after initiation of the experiment (65 days after treatment application in the field, 77 days after treatment application in the greenhouse).

Table 3 - Stem density of flowering shoots (stems·m⁻²) in field plots 59 days after application of oil and/or dispersant to salt marsh vegetation.

	Control	Oil	Dispersant	Oil plus Dispersant
Creek Edge	36.2(58.8)	0.5(0.9)	1.7(2.9)	0
Mid Marsh	7.0(6.1)	1.0(0.9)	0	0
Upper Marsh	51.8(41.3)	18.5(2.2)	0	0.2(0.3)

H₃: There is no interaction (synergistic or inhibitory) between the factors oil and dispersant, affecting S. alterniflora or S. patens cover.

These hypotheses were tested in each of the three vegetation zones. Results of the Student-Newman-Keuls multiple range tests and two-way analysis of variance procedures used to test these hypotheses are summarized in Appendix 5.

Spartina alterniflora or S. patens on average composed over 95% of total species cover in the three vegetation zones, consequently, statistical analysis was restricted to these species. In the creek edge zone, there were no significant pretreatment differences between S. alterniflora cover of plots assigned to the various treatments (Figure 18, Appendix 5). Fifteen days after treatment (day 75) the average cover values of all oil and/or dispersant treated plots were significantly reduced ($\alpha = 0.05$, $df=2$) with oiled plots affected the least and oil plus dispersant treated quadrats affected the most. S. alterniflora cover values for the dispersant and oil plus dispersant treatments continued to decline until 42 days after treatment (day 102), however, the cover values of oiled plots remained stable. S. alterniflora cover values for all oil and/or dispersant treatments were significantly lower than the control at this time. These results suggest that the null hypotheses H₁ and H₂ should be rejected. No significant treatment interactions were noted. The null hypothesis H₃ was therefore retained.

In the midmarsh zone, there were no significant pretreatment differences in S. alterniflora cover values between plots (Figure 19, Appendix 5). The application of treatments caused significant reductions ($\alpha = 0.05$, $df=2$) the cover values of all oil and/or dispersant treated quadrats on all posttreatment sampling dates. Fifteen days after treatment (day 75), oil plus dispersant treated plots had the largest reductions in cover values. Oil and dispersant treated plots were equally affected. S. alterniflora cover values for all oil and/or dispersant treated plots continued to decline until 42 days after treatment (day 102), with extremely large reductions associated with all of them. Based on these results, the null hypotheses H₁ and H₂ were rejected. Two way analysis of variance results for day 102, indicated a significant synergistic enhancement of toxicity when oil and dispersant were applied together. The null hypothesis H₃ was rejected on the basis of these results.

The oil and/or dispersant treatments had no effect on S. patens cover values in the high marsh (Figure 20, Appendix 5). Cover values for all oil and/or dispersant treatments continued to rise over time and none were

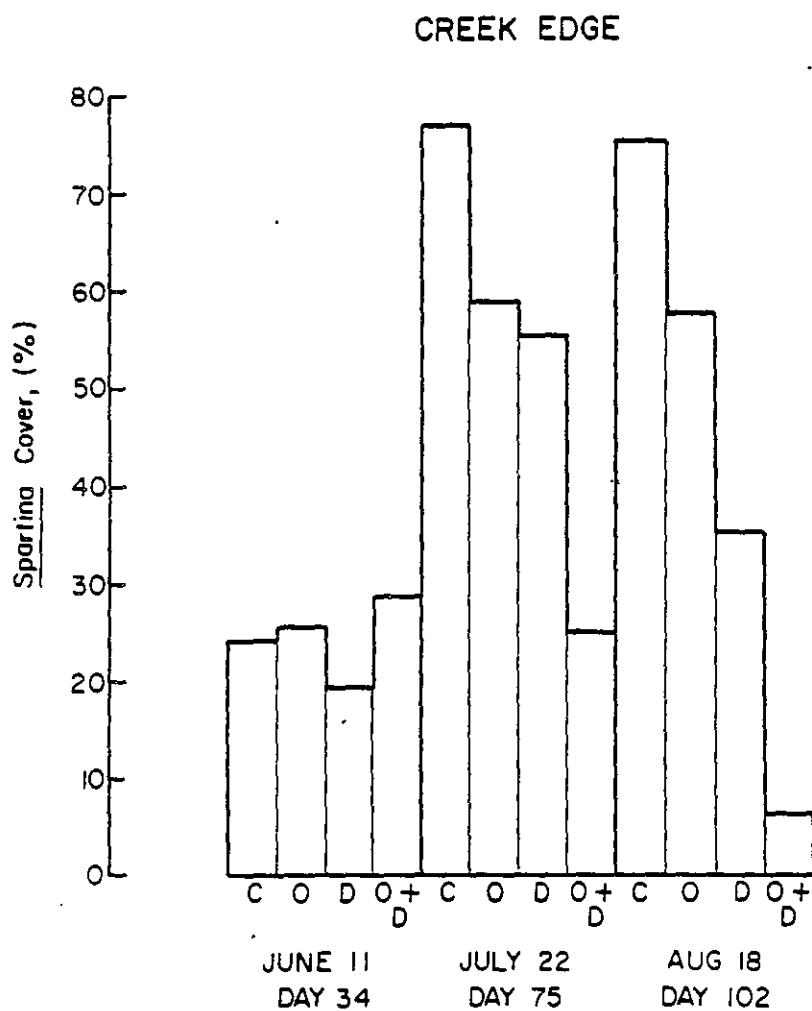


Figure 18: Histogram of mean Spartina alterniflora cover in oil and/or dispersant treated plots from the creek edge zone. Data are from the field study. Treatments were applied on day 60.

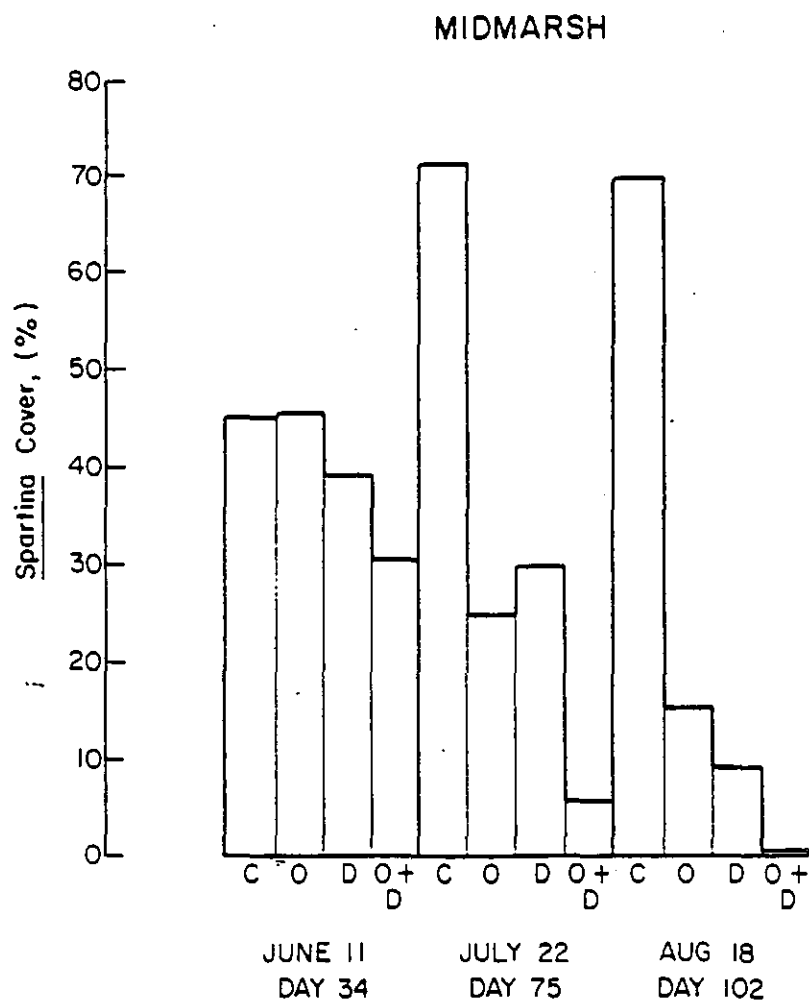


Figure 19: Histogram of mean Spartina alterniflora cover in oil and/or dispersant treated plots from the midmarsh zone. Data are from the field study. Treatments were applied on day 60.

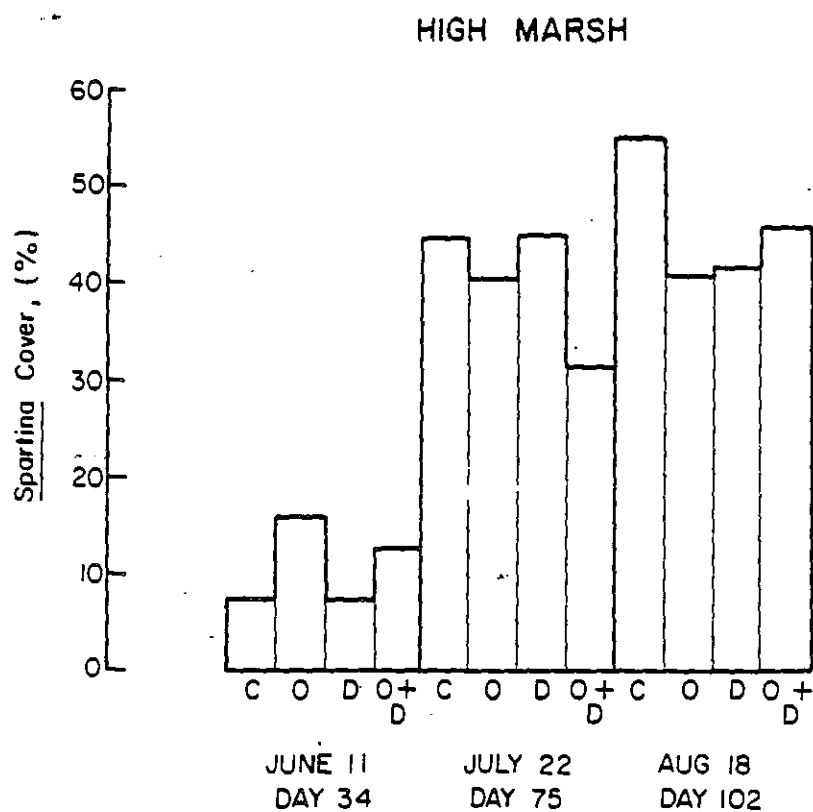


Figure 20: Histogram of mean Spartina alterniflora cover in oil and/or dispersant treated plots from the high marsh zone. Data are from the field study. Treatments were applied on day 60.

significantly different from control values. The null hypotheses H_1 and H_2 were therefore retained. Significant treatment interactions were not found, therefore H_3 was retained.

In summary, the impact of the oil and/or dispersant treatments on Spartina cover was highest in the midmarsh zone and lowest in the high marsh zone. In the creek edge and midmarsh zones, the oil plus dispersant treatment had the most impact on Spartina cover, while oil had the least. In the high marsh zone, there was very little difference between treatments. Significant interactions between oil and dispersant treatments were found only in the midmarsh zone where a synergistic enhancement of toxicity was noted.

Spartina patens cover values in the midmarsh zone tended to increase over time while S. alterniflora cover values declined. In the oil plus dispersant treated plots S. alterniflora was replaced as the dominant species by S. patens. Inspection of the S. patens shoots revealed very few symptoms of oil or dispersant toxicity while the surrounding S. alterniflora shoots exhibited severe stress. The survival of this species outside of its optimal growing conditions while exposed to the toxic effects of oil and/or dispersant treatments suggests that S. patens is particularly tolerant of oil and dispersant.

Species other than Spartina alterniflora and S. patens on average comprised less than 3% cover in all zones (Tables 5-7). Of these species only Salicornia europaea occurred regularly. Salicornia cover responded differently in the various vegetation zones. In the creek edge zone, Salicornia cover values declined in the oil and oil plus dispersant treated plots and remained stable in the dispersant treated plots. Salicornia cover values, however, also declined in the control plots making it impossible to determine whether natural mortality or oil and/or dispersant toxicity was responsible for the declines in the treated plots. Decreases in control plot Salicornia cover were also noted in the high marsh zone. Salicornia cover in the high marsh oil and/or dispersant treated plots appeared unaffected by the treatments. In the midmarsh zone, Salicornia cover remained stable in all treatments. The overall impression from these results is that Salicornia cover was not affected by the treatments. These results are contrary to the results of Baker et al. (1984) who found Salicornia europaea to be sensitive to both oil and dispersant.

Leaf Anatomy - The development of microscopic symptoms appeared to be identical for all species and for all treatments. As the conditions of plants deteriorated, the number of chloroplasts within the outer bundle sheath and mesophyll cells declined and eventually disappeared.

Table 5 - Species composition of control, oil and/or dispersant treated field plots in the creek edge zone on three sampling dates. Treatments were applied on day 60.

Species	Treatment	Average Cover (1 Standard Deviation)		
		June 11 Day 34	July 22 Day 75	Aug. 18 Day 102
<u>Spartina alterniflora</u>	Control	24.2(3.8)	77.1(6.9)	75.4(7.4)
	Oil	25.4(6.4)	58.0(7.3)	57.5(8.8)
	Dispersant	18.9(3.0)	55.4(11.4)	35.4(8.9)
	Oil+Dispersant	28.1(8.0)	24.6(1.9)	6.6(4.4)
<u>Spartina patens</u>	Control	0	0	0
	Oil	0.1(0.2)	0	0.4(0.8)
	Dispersant	0	0	0.2(0.3)
	Oil+Dispersant	0	0	0.1(0.1)
<u>Salicornia europaea</u>	Control	0.4(0.4)	0.2(0.2)	0
	Oil	0.1(0.1)	0	0
	Dispersant	0.2(0.2)	0.3(0.5)	0.2(0.3)
	Oil+Dispersant	0.3(0.3)	0.1(0.2)	0.1(0.1)
<u>Suaeda maritima</u>	Control	0	0	0
	Oil	0	0	0
	Dispersant	0.1(0.1)	0	0
	Oil+Dispersant	0.1(0.2)	0	0
<u>Plantago juncooides</u>	Control	0	0	0
	Oil	0	0	0
	Dispersant	0.7(1.2)	2.1(3.6)	2.4(4.2)
	Oil+Dispersant	0	0	0
<u>Atriplex patula</u>	Control	0	0	0
	Oil	0	0	0
	Dispersant	0.1(0.1)	0	0
	Oil+Dispersant	0	0	0

Table 6 - Species composition of control, oil and/or dispersant treated field plots in the midmarsh zone on three sampling dates. Treatments were applied on day 60.

Species	Treatment	Average Cover (1 Standard Deviation)		
		June 11 Day 34	July 22 Day 75	Aug. 18 Day 102
<u>Spartina alterniflora</u>	Control	45.2(21.4)	71.3(9.9)	69.6(11.8)
	Oil	45.8(8.0)	25.0(11.9)	15.4(12.1)
	Dispersant	39.3(22.5)	29.8(14.8)	8.6(8.9)
	Oil+Dispersant	30.8(5.8)	5.8(4.0)	0.4(0.2)
<u>Spartina patens</u>	Control	0.2(0.3)	0.4(0.8)	1.8(3.2)
	Oil	0	0	0.5(0.8)
	Dispersant	0	0	0.6(0.5)
	Oil+Dispersant	0	0	0.7(0.7)
<u>Salicornia europaea</u>	Control	0.2(0.1)	0.1(0.2)	0.4(0.1)
	Oil	0.2(0.2)	0.2(0.3)	0.2(0.2)
	Dispersant	0.3(0.3)	0.3(0.2)	0.3(0.3)
	Oil+Dispersant	0.5(0.4)	0.3(0.2)	0.2(0.2)
<u>Suaeda maritima</u>	Control	0	0	0.1(0.1)
	Oil	0.1(0.1)	0	0
	Dispersant	0.1(0.2)	0	0
	Oil+Dispersant	0	0	0
<u>Triglochin elata</u>	Control	0.3(0.3)	0	0.2(0.3)
	Oil	0.1(0.1)	0	0.1(0.1)
	Dispersant	0	0	0
	Oil+Dispersant	0	0	0
<u>Plantago juncooides</u>	Control	0	0	0
	Oil	0	0	0
	Dispersant	0	0	0.1(0.2)
	Oil+Dispersant	0	0	0
<u>Limonium nashii</u>	Control	0	0	0
	Oil	0	0.1(0.1)	0
	Dispersant	0	0	0
	Oil+Dispersant	0	0	0

Accompanying this was the appearance of orange granules in these cells. These may have been the remains of chloroplasts or residual photosynthetic pigments such as carotene. Concomitant to the progression of these microscopic symptoms, was the development of chlorosis at the macroscopic level. Plasmolysis or erosion of the cuticle were not observed in any of the samples.

There are several ways in which oil can affect plants (Baker 1970, 1971a). It can act physically by blocking stomata, thereby interfering with gas exchange and transpiration. Oil, which enters the plant can damage cell membranes resulting in ion "leakage" and osmoregulatory problems (Hutchinson, 1979). Evidence of osmoregulatory damage (overactive salt glands) were noted for oiled plants in the midmarsh zone. Disruption of membranes allows oil to enter cells where it can damage chloroplasts, thereby reducing photosynthetic rates. The evidence presented here suggests that chloroplasts were probably damaged by the oil treatment. Very little is known of the mode of dispersant toxicity in plants. Animal research indicates that dispersants may have their initial effect upon the outer cell membrane, solubilizing the lipid components of membranes and lowering cell surface tension (Wells, 1984). The sites of toxicity for the oil and dispersant are similar and may explain the similarity of microscopic symptoms observed in this study.

Greenhouse Experiment

General Observations - General observations regarding plant health were made at frequent intervals in the greenhouse. The trends noted in these observations were corroborated by less frequent observations in the field.

Plant stress symptoms were generally similar in all three vegetation zones, with the single exception that the symptoms progressed at different rates in different vegetation zones. Symptoms progressed fastest in the midmarsh zone and slowest in the high marsh zone. In the oil treatments in all vegetation zones, oiled portions of leaves became chlorotic within three days. Chlorosis was initially restricted to oiled portions, and unoiled tissues remained apparently healthy. Seven days after oiling, plants in the midmarsh samples became encrusted with salt, suggesting disruption of osmoregulatory functions. This symptom was restricted to the midmarsh samples. With time, chlorosis and eventual death of oiled tissues progressed up the stems and plant mortality continued to increase with time. Twenty days after oiling, 47% of plants in the midmarsh were judged to be dead, while in the creek edge and high marsh zones, only 5% of plants had died. After 68 days, mortality had increased to 79% in the midmarsh, 34% in the high marsh and 23% in the creek edge.

Dispersant-treated plants became slightly chlorotic (yellow) over the entire plant within three days of application. Within five days, the leaf tips had become severely chlorotic (white), and this chlorosis proceeded down the leaves over the following days, moving fastest in the midmarsh samples and slowest in the high marsh samples. Chlorotic areas died within a few days, and by day 20, over 87% of the plants had died in the midmarsh, 20% in the creek edge and 8% in the high marsh.

Combined oil plus dispersant application resulted in a combination of the symptoms described for plants treated with oil alone or dispersant alone. In all zones, symptoms were evident within three days after treatment. Oiled portions of leaves became chlorotic and eventually affected tissues died, while unoiled portions became slightly chlorotic as in dispersant treated samples. Eventually, the leaves became chlorotic at the tips and died back. Mortality after 20 days was similar to that of dispersant alone treatments with 90% in the midmarsh samples, 27% in the creek edge and 11% in the high marsh.

Two kinds of deformities of plant morphology were observed in high marsh S. patens, treated with either oil or with the oil plus dispersant mixture. The first was the occurrence of wavy ridges on the adaxial leaf surfaces. The second abnormality was more obvious, and was found in approximately 10% of the plants. In these, a U-shaped crook or loop developed in the stems, usually occurring immediately above the oiled portions of the plants. Microscopic comparison of deformed and normal stems revealed that the inner layers of leaves were almost fully developed in the deformed stems. (A grass shoot consists of concentric layers of leaves in various stages of development.) All layers in the deformed plants appeared to be developmentally mature while in the normal stems the inner layers were much less developed than the outer layers. A component of the oil may mimic or interfere with the production or reception of plant growth hormones, thereby causing these deformities. Boney (1974) noted that certain polynuclear aromatic hydrocarbons appeared to be able to affect growth form in macroalgae, possibly through altering normal apical growth of the plants.

Plant Height - The null hypotheses tested in this experiment were:

- H₁: Oil has no effect on Spartina alterniflora or S. patens height growth.
- H₂: Dispersant has no effect on Spartina alterniflora or S. patens height growth.

Table 7 - Species composition of control, oil and/or dispersant treated field plots in the high marsh zone on three sampling dates. Treatments were applied on day 60.

Species	Treatment	Average Cover (1 Standard Deviation)		
		June 11 Day 34	July 22 Day 75	Aug. 18 Day 102
<u>Spartina patens</u>	Control	7.6(1.2)	44.2(0.7)	54.2(1.4)
	Oil	16.2(14.5)	40.0(21.4)	40.4(9.1)
	Dispersant	6.9(2.7)	44.6(13.7)	41.3(5.8)
	Oil+Dispersant	12.5(10.2)	31.3(11.5)	45.4(10.7)
<u>Spartina alterniflora</u>	Control	0	0.2(0.3)	0.3(0.5)
	Oil	0	0	0
	Dispersant	0	0	0.1(0.2)
	Oil+Dispersant	0.5(0.6)	1.7(2.7)	0.7(0.7)
<u>Salicornia europaea</u>	Control	0.2(0.3)	0.1(0.2)	0
	Oil	0.7(0.3)	0.6(0.4)	0.4(0.4)
	Dispersant	0.2(0.3)	0.4(0.3)	0.3(0.4)
	Oil+Dispersant	0	0	0
<u>Triglochin elata</u>	Control	0	0	0
	Oil	0.2(0.2)	0	0
	Dispersant	0.1(0.1)	0	0
	Oil+Dispersant	0	0	0
<u>Suaeda maritima</u>	Control	0	0	0
	Oil	0.1(0.1)	0.1(0.1)	0
	Dispersant	0	0	0
	Oil+Dispersant	0	0	0
<u>Atriplex patula</u>	Control	0	0	0
	Oil	0	0.1(0.1)	0.1(0.1)
	Dispersant	0	0	0
	Oil+Dispersant	0	0	0
<u>Plantago juncooides</u>	Control	0	0	0
	Oil	0	0	0
	Dispersant	0	0	0
	Oil+Dispersant	0.1(0.2)	0.1(0.2)	0

Table 7 (continued)

<u>Glaux maritima</u>	Control	0	0	0
	Oil	0	0.1(0.1)	0.2(0.3)
	Dispersant	0	0	0
	Oil+Dispersant	0	0	0
<u>Limonium nashii</u>	Control	0	0	0
	Oil	0	0.1(0.1)	0
	Dispersant	0	0	0
	Oil+Dispersant	0	0	0

H₃: There is no interaction (synergistic or inhibitory) between the factors oil and dispersant, affecting S. alterniflora or S. patens height growth.

These hypotheses were tested in each of the three vegetation zones. Results of the Student-Newman-Keuls multiple range tests and two-way analysis of variance procedures used to test these hypotheses are summarized in Appendices 6 and 7.

There were significant differences in the pretreatment plant heights among treatments in the creek edge zone (Figure 21, Appendix 6). Plants slated for the oil plus dispersant treatment were significantly taller ($\alpha = 0.05$, $df=4$) than plants slated for oil and dispersant treatments on all pretreatment sampling dates. Plants slated for the dispersant treatment were significantly shorter ($\alpha = 0.05$, $df=4$) than the control plants on all pretreatment sampling dates except day 27. After treatment applications all oil and/or dispersant treatments suffered similar reductions in height growth for a period of at least 25 days, however, because of the pretreatment differences in plant height, only the oil and dispersant treated plants were significantly shorter ($\alpha = 0.05$, $df=4$) than the control plants. Between days 68 and 82, however, there was a rapid increase in mean height in all oil and/or dispersant treatments. At this point, only the dispersant treated plants were significantly shorter ($\alpha = 0.05$, $df=4$) than the controls. These increases were probably attributable to higher mortality of small plants. Mean heights for dispersant and oil plus dispersant treated plants remained high between days 82 and 91, then declined as larger plants eventually succumbed and were replaced by new shoots. Mean height for oiled plants, however, continued to rise through the experiment and was not significantly different from the control on days 82, 91 and 117.

In the midmarsh zone, the oil treatment had no significant effect on plant height (Figure 22, Appendix 6). The oil plus dispersant treatment had no apparent impact on average plant height until day 68 (25 days after treatment), after which average plant height was significantly reduced ($\alpha = 0.05$, $df=4$) for the rest of the experiment. The dispersant treatment was associated with significant reductions ($\alpha = 0.05$, $df=4$) in plant height from day 41 (2 days before treatment application) to the end of the experiment. Contrary to other parameters investigated, the oil and/or dispersant treatments appeared to have less impact in the midmarsh zone than in the creek edge zone. Ironically, this discrepancy is probably attributable to the heavy mortality associated with the oil and/or dispersant treatments in the midmarsh zone. Mortality of tagged plants 12 days after treatment (day 55) were 18% for oiled plants, 70% for plants

receiving the dispersant treatment and 60% for plants treated with oil plus dispersant (Table 8). Mortality of the oil treated plants was relatively light initially, however, small plants tended to die first, thereby increasing the average height of surviving plants which compensated for the significant reduction ($\alpha = 0.05$, $df=4$) of height growth associated with this treatment. The rapid loss of most of the plants in the oil and/or dispersant treatments restricted the selection of replacement plants to those located at the outside edges of the microcosms. These plants were exposed to less oil and/or dispersant and were therefore less impacted than the majority of plants in the microcosms. These plants were able to maintain (at least temporarily) growth rates similar to the controls. Eventually, these plants were affected by the oil and/or dispersant and their growth rates declined. In addition, small numbers of new shoots emerged and their heights tended to further reduce average plant height as occurred in the dispersant and oil plus dispersant treated microcosms after day 68.

Pretreatment mean plant heights for high marsh microcosms slated for the dispersant treatment were significantly taller ($\alpha = 0.05$, $df=4$) than all other treatments (Figure 23, Appendix 6). The control, oil and oil plus dispersant treatments, however, were not significantly different from each other. Twelve days after treatment applications (day 55), all treatments caused reductions in plant height, however, only the oil treatment caused a significant reduction ($\alpha=0.05$, $df=4$) in plant height. Within 25 days of treatment application (day 68), all treatments had caused significant reductions ($\alpha=0.05$, $df=4$) in plant height. Mean plant height for oiled plants recovered to control values within 39 days of treatment (day 82) and remained at control levels for the rest of the experiment. Between 25 and 48 days after treatment (days 68 and 91), dispersant treated plants remained shorter than control plants. They were significantly shorter ($\alpha 0.05$, $df=4$), however, only on days 68 and 82. Seventy-four days after treatment applications, the mean height of dispersant treated plants was similar to that of the mean height of control plants. The mean height of oil plus dispersant treated plants remained lower than control plants through the post-treatment portion of the experiment. This reduction was significant ($\alpha=0.05$, $df=4$) on all sampling dates except day 117.

Rapid fluctuations in the mean heights of oil and/or dispersant treated plants were not noted in the high marsh zone, probably as a result of the relatively low plant mortality rates associated with this zone. Large modifications of size structure caused by size dependent mortality (as noted in the creek edge and midmarsh zones) were not found in the high marsh zone.

Table 8: Percentage of original tagged plants alive on each sampling date.
Treatments were applied on day 43.

Percent Surviving (1 standard deviation)						
Treatment	Day 41	Day 55	Day 68	Day 82	Day 91	Day 117
Creek Edge Zone						
Control	100(0)	96(6)	94(6)	86(9)	86(9)	76(18)
Oil	100(0)	88(13)	72(16)	54(15)	52(11)	30(10)
Dispersant	100(0)	88(13)	80(10)	58(21)	54(15)	2(5)
Oil plus Dispersant	100(0)	66(18)	52(24)	36(11)	32(8)	12(5)
Midmarsh Zone						
Control	100(0)	92(5)	88(5)	84(9)	80(12)	66(20)
Oil	100(0)	82(19)	30(7)	20(10)	16(13)	12(13)
Dispersant	98(5)	30(42)	16(31)	2(4)	0(0)	0(0)
Oil plus Dispersant	100(0)	40(28)	0(0)	0(0)	0(0)	0(0)
Highmarsh Zone						
Control	100(0)	100(0)	98(5)	96(9)	92(8)	82(13)
Oil	100(0)	98(5)	78(22)	70(24)	64(21)	58(22)
Dispersant	100(0)	96(6)	90(7)	88(8)	76(21)	46(25)
Oil plus Dispersant	96(6)	84(9)	66(6)	58(11)	52(11)	40(16)

HIGH MARSH (Greenhouse)

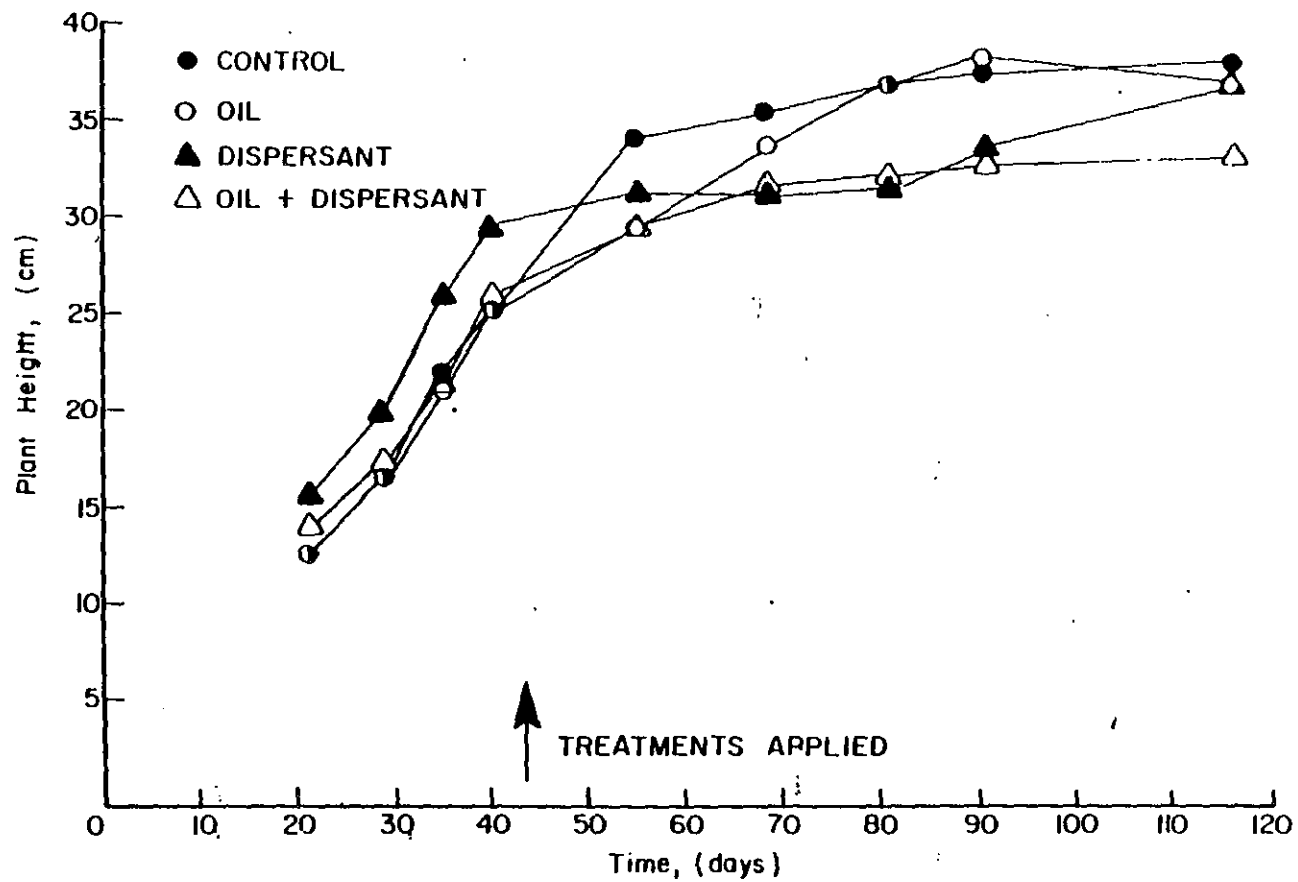


Figure 23: Mean *Spartina patens* shoot height for the high marsh zone versus time. Data are from the greenhouse study.

The use of tagged plants allowed the determination of plant growth rates. Figure 24 (Appendix 7) presents growth rate data for control, oil and/or dispersant treated plants in the creek edge zones over the course of the experiment. Growth rates were significantly reduced ($\alpha = 0.05$, $df=4$) by all treatments for approximately 40 days after treatment (day 43 to day 82). During this period dispersant reduced growth the most and oil the least. Between 39 and 48 days after treatment (days 82-91) only dispersant treated plants had significantly reduced ($\alpha=0.05$, $df=4$) growth rates, while 48 to 74 days after treatment (days 91-117) dispersant treated plants grew significantly slower than controls while oil plus dispersant treated plants grew significantly faster. Two-way analysis of variance indicated that oil significantly inhibited ($\alpha = 0.01-0.05$, $df=4$) dispersant toxicity through the experiment. The meristematic tissues of grasses are located at ground level. Oil deposits on the soil surface may have shielded the meristematic tissues from the relatively more toxic dispersants allowing these plants to maintain higher growth rates. Growth rates of oil plus dispersant treated plants increased between 39 and 74 days after treatment (days 82-117) and were significantly higher ($\alpha = 0.05$, $df=4$) than controls during the last sampling period indicating an apparent amelioration of toxic effects. As Table 8 indicates, however, the oil plus dispersant treatment was responsible for the largest mortality rates among the tagged plants, negating the long term beneficial effect it had on growth rates.

These data indicate that both oil and dispersant were responsible for reductions in Spartina alterniflora height growth in the creek edge zone, therefore the null hypotheses H_1 and H_2 were rejected. There were significant inhibitory interactions between the oil and dispersant, therefore the null hypothesis H_3 was retained.

In the midmarsh zone (Figure 25, Appendix 7), significant reductions ($\alpha = 0.05$, $df=4$) of growth rate were noted for the oil and dispersant treatments during the first 12 days after treatment (days 43 - 55). During this period, the dispersant treatment caused the largest reduction in growth rate while the oil plus dispersant treatment caused the least reduction in growth rate.

As in the creek edge zone, height growth benefits associated with the oil plus dispersant treatment were outweighed by the high mortality rates associated with this treatment. All tagged plants in the oil plus dispersant treated microcosms were dead within 25 days of treatment ((day 68) Table 8). Two-way analysis of variance indicated a significant ($\alpha = 0.01$, $df=4$) inhibitory interaction between the oil and dispersant. Oil may have coated the meristematic tissues, protecting them from the relatively more toxic dispersant. The fact that growth rates for oil plus dispersant

CREEK EDGE

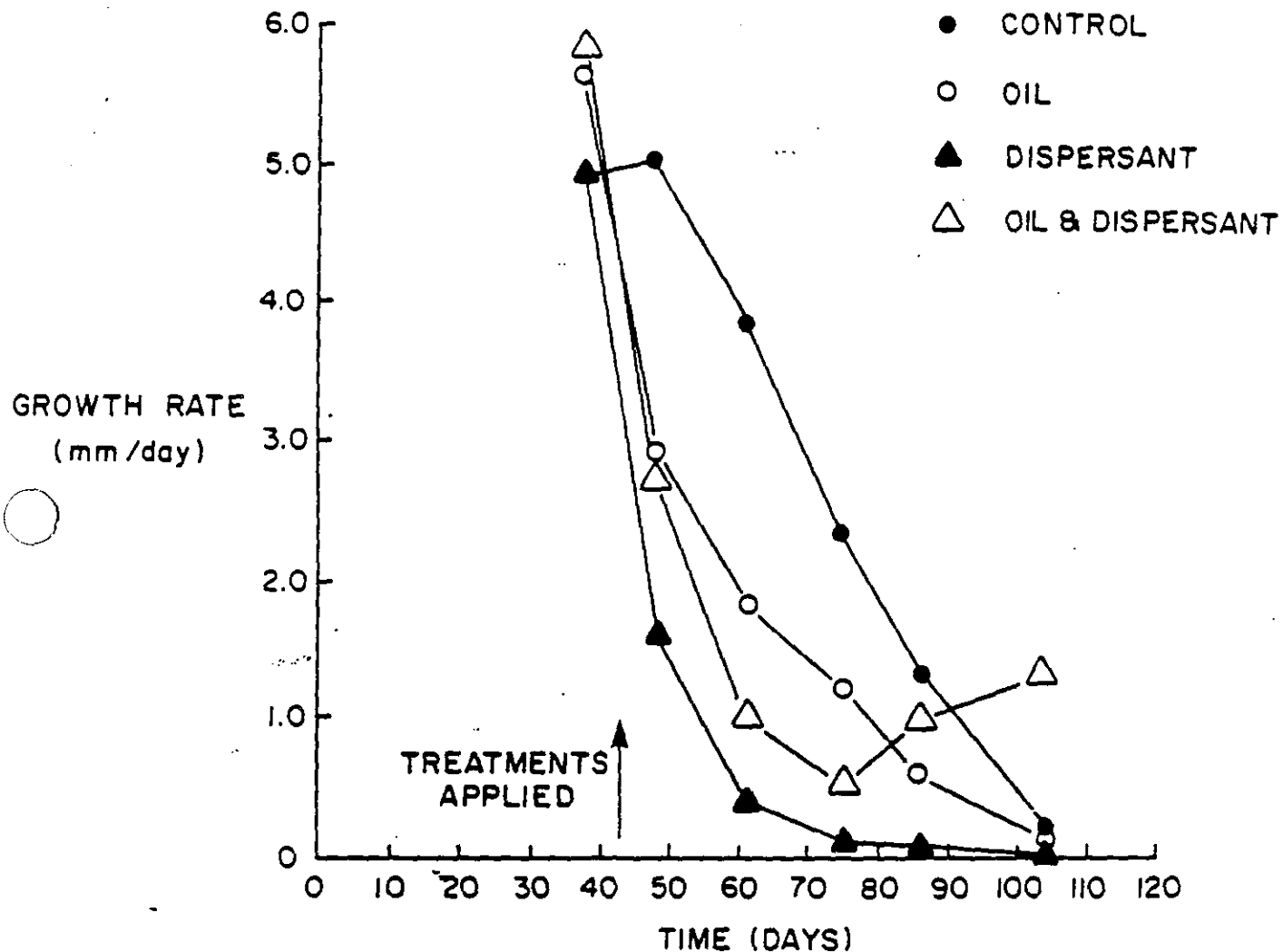


Figure 24: Mean growth rate of control, oil and/or dispersant treated plants from the creek edge microcosms versus time.

MIDMARSH

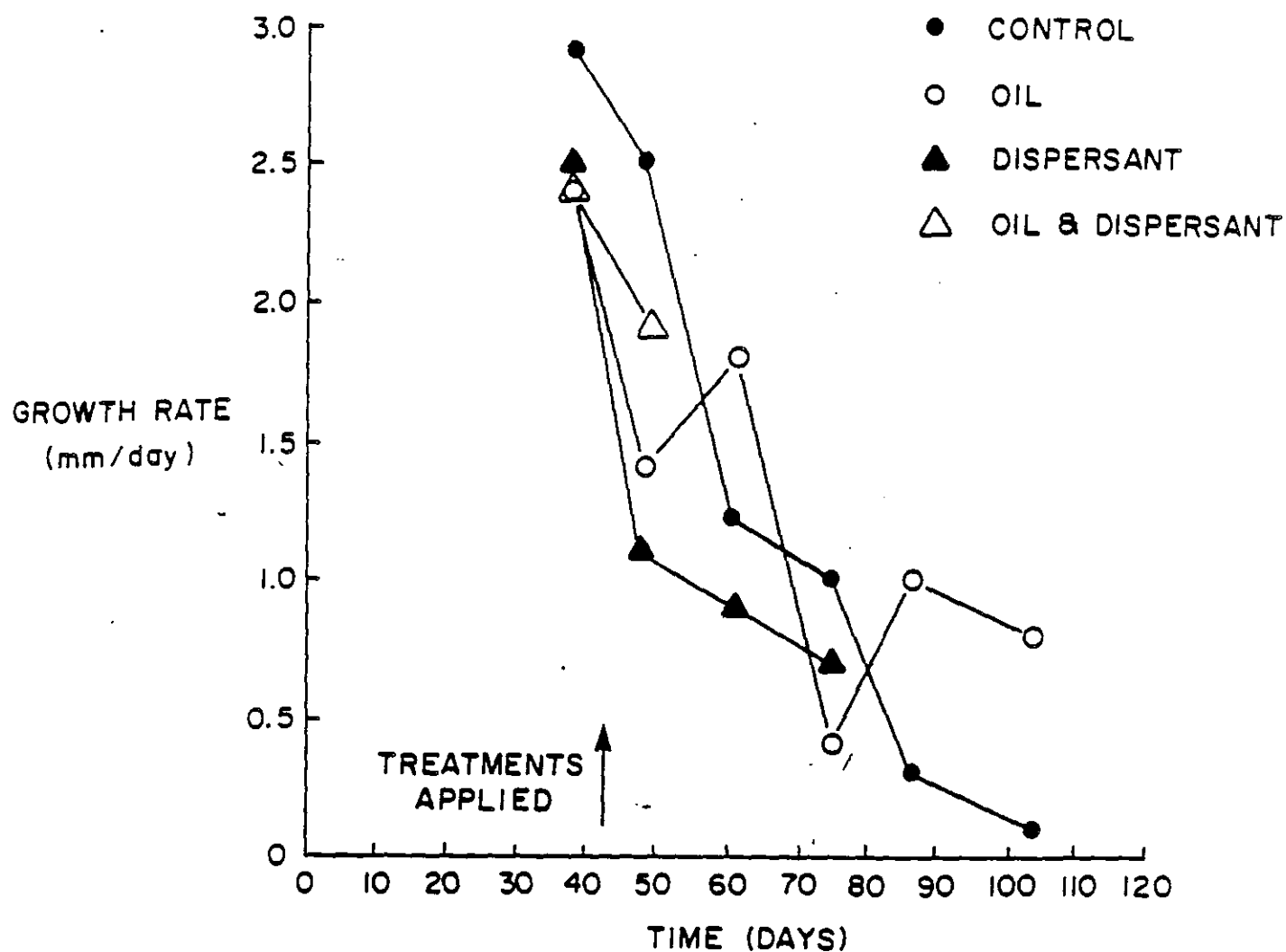


Figure 25: Mean growth rates of control, oil and/or dispersant treated plants from the midmarsh microcosms versus time. No data were available for oil plus dispersant treated plants beyond day 55 and beyond day 82 for dispersant treated plants as a result of mortality of all tagged plants.

HIGH MARSH

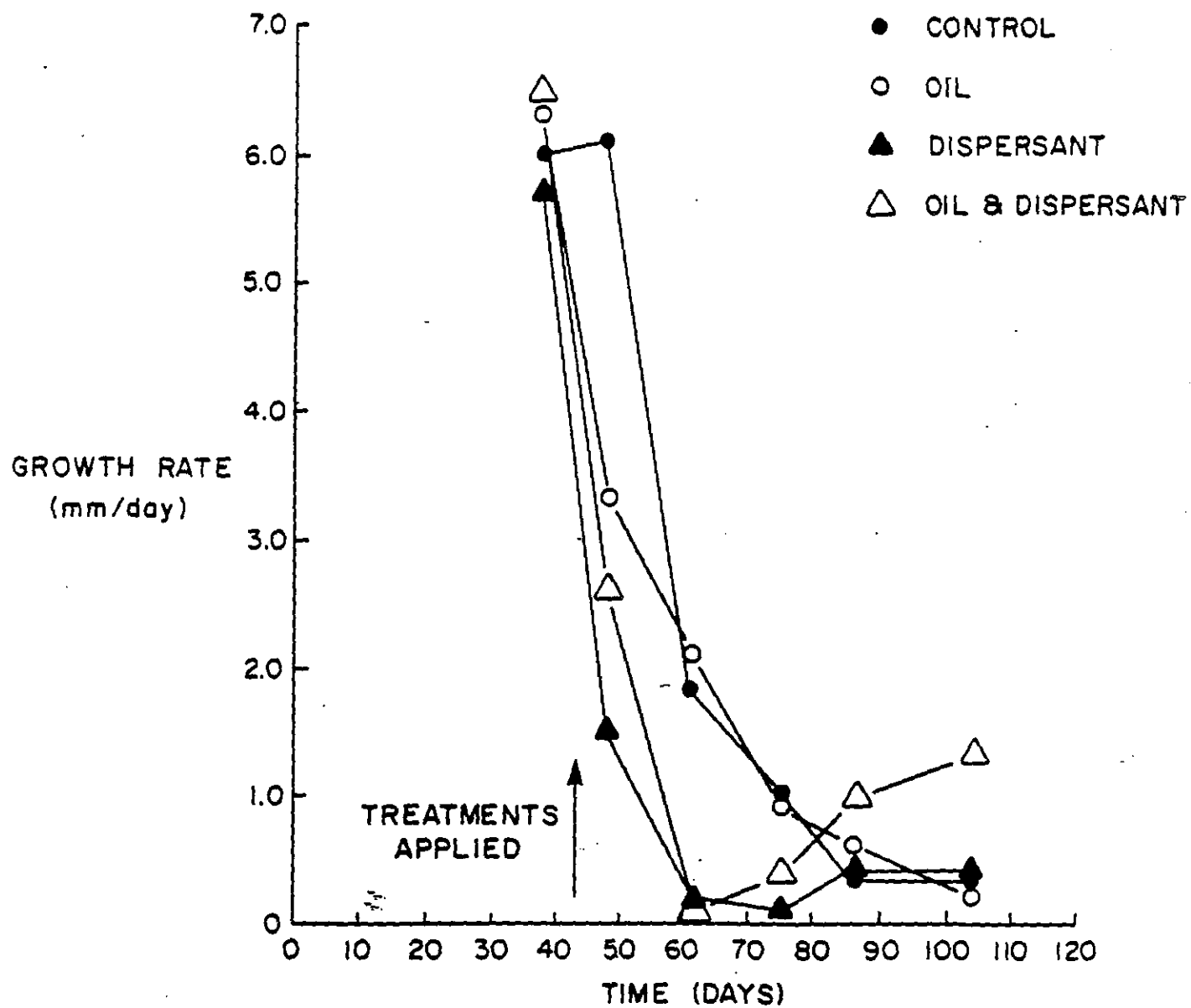


Figure 26: Mean growth rates of control, oil and/or dispersant treated plants from the high marsh microcosms versus time.

treated plants were higher than the growth rates of oiled plants may be attributable to the erratic fluctuations of growth rates exhibited by oiled plants. Growth rates for dispersant treated plants were consistently lower than those of controls, however they were significantly lower ($\alpha = 0.05$, $df=4$) only between 0 and 12 days after treatment (days 43 and 55) and 25 to 39 days after treatment (days 68 to 82). All tagged dispersant treated plants had died within 39 days of treatment ((day 82) Table 8). Growth rates for oiled plants were highly variable and were significantly different from the controls only within 12 days of treatment (days 43 to 55).

Growth rate data indicate that the oil treatment had a significant acute impact on plant height growth, however, both average plant height and growth rate data suggest that no long term effects occurred. Based on these results, the null hypothesis H_1 was retained. Both average plant height and growth rate data indicate long term impacts on Spartina alterniflora growth associated with the dispersant treatment. The null hypothesis H_2 was, therefore, rejected. Significant inhibitory interactions between oil and dispersant were noted in both the average height and growth rate data indicating that the null hypothesis H_3 should be rejected.

In the high marsh zone, the oil treatments had the least impact on growth rates (Figure 26, Appendix 7). Oil treatments significantly reduced ($\alpha = 0.05$, $df=4$) growth rates during the first 12 days following treatment applications, however, no significant differences were found in the growth rates of oiled and control plants on any other sampling date. The dispersant treatment caused the largest reductions in growth rates. Significant growth rate reductions ($\alpha = 0.05$, $df=4$) associated with the dispersant treatment persisted for approximately 40 days after treatment application (days 43 to 82). After this period growth rates of dispersant treated plants recovered to control levels. Growth rates for oil plus dispersant treated plants were generally intermediate between the growth rates of oil and dispersant treated plants during the first 40 days following treatment, then quickly recovered and had the highest growth rates by the end of the experiment. Significant reductions ($\alpha = 0.05$, $df=4$) in growth rate associated with the oil plus dispersant treatment were noted during the first 25 days following treatment applications. During the last sampling period (days 91 to 117), growth rates for oil plus dispersant treated plants were significantly higher than all other treatments. Two-way analysis of variance indicated a significant inhibitory interaction ($\alpha = 0.01$, $df=4$) between the oil and dispersant during the first 12 days following treatment (days 43 to 55) and the last 26 days of the experiment (days 91 to 117). As in the creek edge and

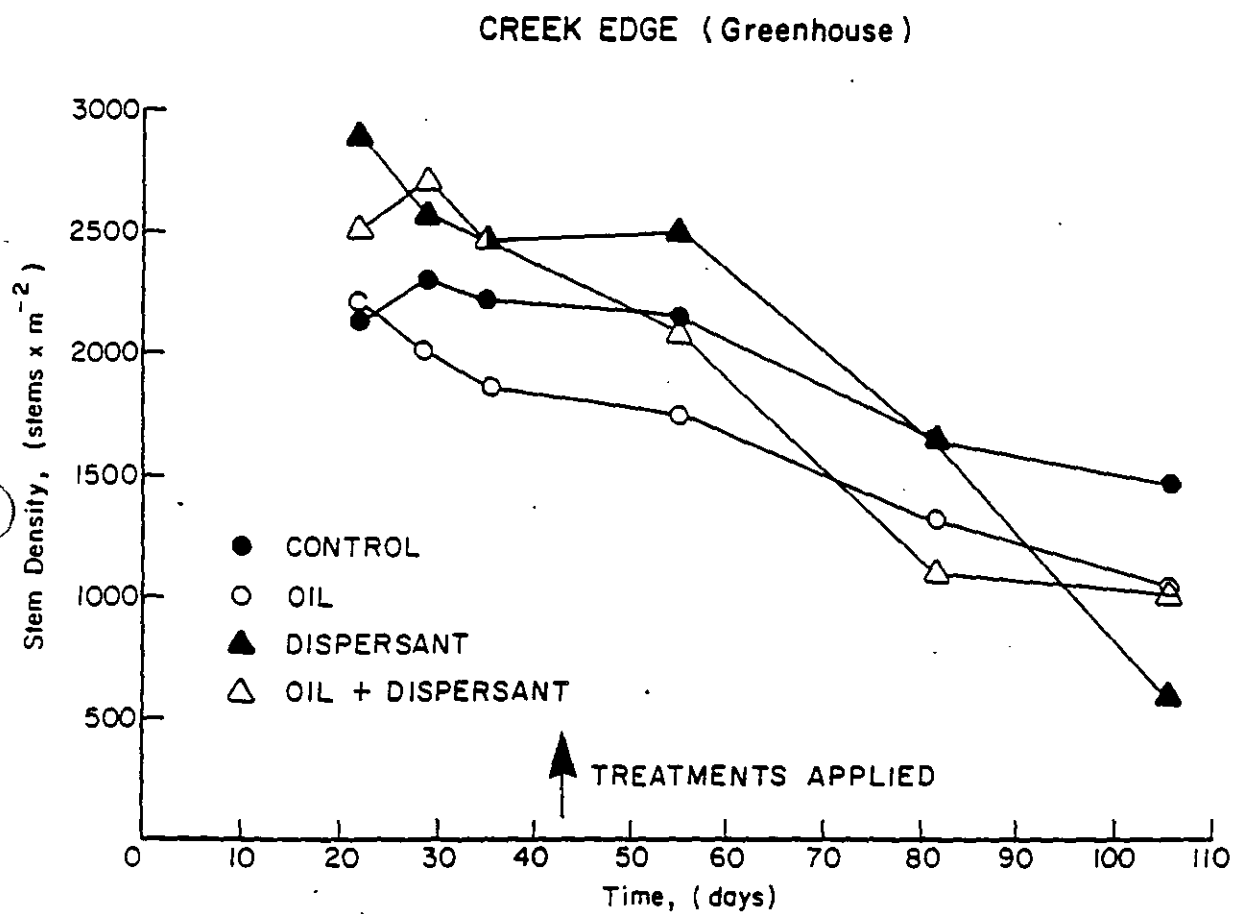


Figure 27: Mean stem density of *Spartina alterniflora* versus time. Data from control, oil and/or dispersant treated creek edge zone greenhouse microcosms are presented. Treatments were applied on day 43.

midmarsh zones, a layer of oil deposited around the meristematic tissues may have protected them from the relatively more toxic dispersant. The late increase in growth rates associated with the oil plus dispersant treatment may indicate an amelioration of toxicity. This benefit was outweighed by the increased mortality associated with this treatment (Table 8).

The results of both the average plant height and growth rate data indicate a temporary reduction in plant growth in the oiled microcosms followed by a rapid recovery to control levels. These results indicate that the null hypothesis H_1 should be retained. The data suggests long term impacts on plant growth for dispersant treated plants, therefore, the null hypothesis H_2 was rejected. The presence of significant inhibitory interactions indicated that the null hypothesis H_3 should be rejected.

Stem Density - The null hypotheses tested in this experiment were:

- H_1 : Oil has no effect on Spartina alterniflora or S. patens stem density.
- H_2 : Dispersant has no effect on Spartina alterniflora or S. patens stem density.
- H_3 : There is no interaction (synergistic or inhibitory) between the factors oil and dispersant, affecting S. alterniflora or S. patens stem density.

These hypotheses were tested in each of the three vegetation zones. Results of the Student-Newman-Keuls multiple range tests and two-way analysis of variance procedures used to test these hypotheses are summarized in Appendix 8.

Oiling of creek edge vegetation caused no significant reductions in stem density through the growing season (Figure 27, Appendix 8). The dispersant treatment caused a progressive reduction in stem density which was significantly lower ($\alpha = 0.05$, $df=4$) than the control only on the last sampling date (day 106). The oil plus dispersant treatment also resulted in a progressive decline in stem density, however, stem density was not significantly different from the control at any time. For both of these treatments, stem density did not begin to decline until at least two weeks after treatment application. The rate of stem density decline was much faster in the dispersant and oil plus dispersant treated microcosms than in the control or oil treated microcosms. Initial stem density in the dispersant and oil plus dispersant microcosms were higher than those of the control and oil microcosms, thereby masking the different rates of stem density reduction. Based on these results, the null hypothesis H_1 was

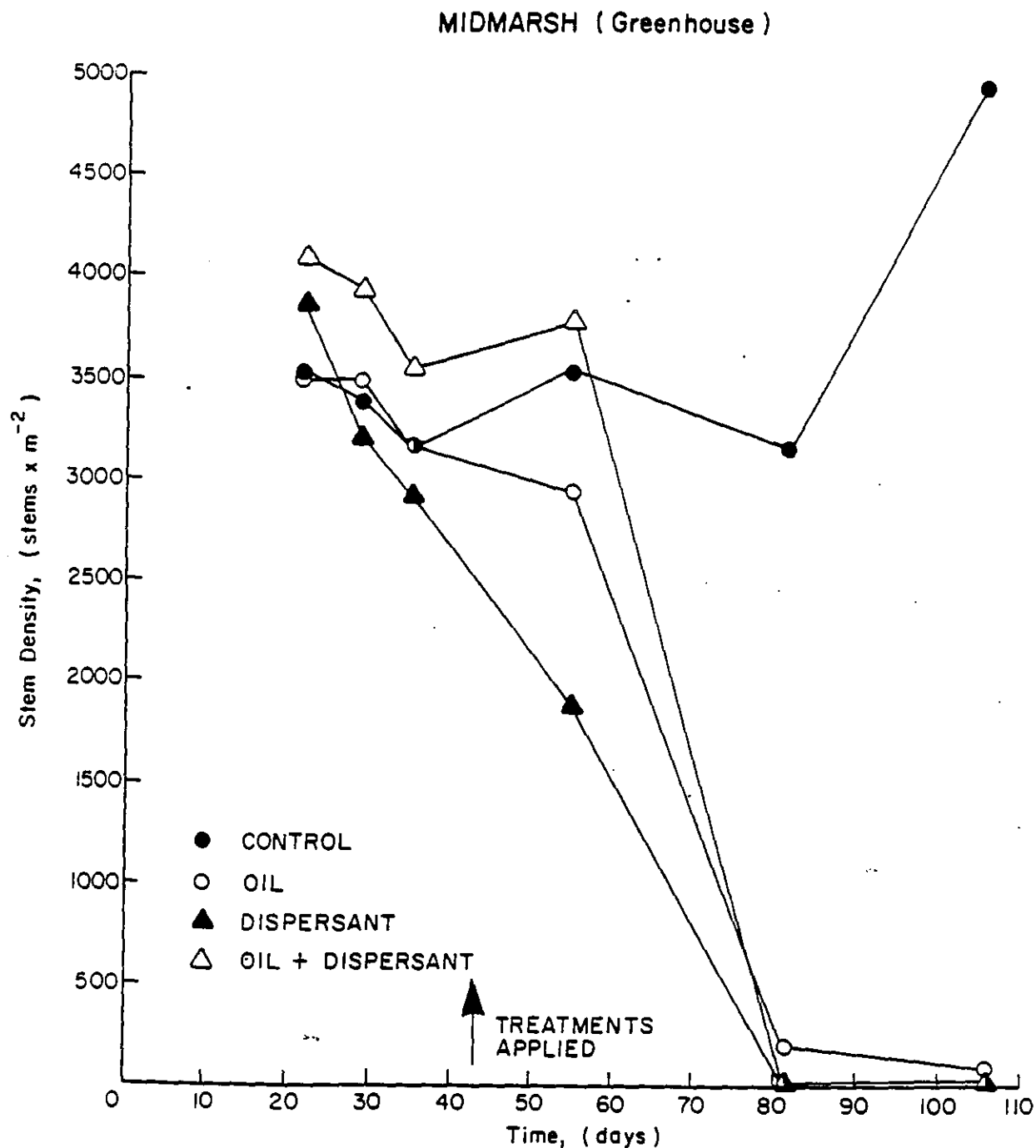


Figure 28: Mean stem density of *Spartina alterniflora* versus time. Data from control, oil and/or dispersant treated midmarsh zone greenhouse microcosms are presented. Treatments were applied on day 43.

retained while H_2 was rejected. No significant interactions were noted between the oil and dispersant treatments. The null hypothesis H_3 was therefore rejected.

In the midmarsh zone all oil and/or dispersant treatments caused significant reductions ($\alpha = 0.05$, $df=4$) in stem density from 39 days after treatment applications (day 82) to the end of the sampling period (day 106) (Figure 28, Appendix 8). Dispersant and oil plus dispersant almost eliminated Spartina alterniflora from the midmarsh microcosms. Oil caused only slightly less mortality of stems. Mortality in the dispersant treated microcosms was evident within one week of treatment application, however, in the oil and oil plus dispersant microcosms it was delayed at least two weeks. Oil appeared to temporarily inhibit the toxic effects of the dispersant, probably by forming a film over the plants which neutralized dispersant contacting the plant surface or reducing the uptake of the dispersant by plugging or stimulating the closure of stomata. Based on these results, the null hypotheses H_1 and H_2 were rejected. Significant treatment interactions were not observed in the midmarsh zone. The null hypothesis H_3 was therefore retained.

Stem density in the high marsh zone was affected most by the oil and oil plus dispersant treatments (Figure 29, Appendix 8). Stem densities for quadrats receiving these treatments were significantly lower ($\alpha = 0.05$, $df=4$) than the controls on the last two sampling dates (days 82 and 106), although there was a small recovery of stem density between days 82 and 106. Stem density reductions were not evident until at least two weeks after treatment applications. The dispersant treatment had no significant impact on stem density. Based on these results, the null hypothesis H_1 was rejected while H_2 was retained. Two-way analysis of variance results for the high marsh zone revealed no significant interactions between the oil and dispersant, therefore, the null hypothesis H_3 was retained.

Comparison of the three vegetation zones revealed that the midmarsh zone was the most sensitive to the oil and/or dispersant treatments. The creek edge appeared to be the most tolerant zone, however, when the differences in pretreatment stem density were taken into consideration, the treatment impacts on this zone and the high marsh zone were approximately equal.

Treatments varied in toxicity between vegetation zones. In Spartina alterniflora dominated zones (creek edge, and midmarsh) stem density was most reduced by either the dispersant or oil plus dispersant treatments which were approximately equal in impact. Oil had the least impact although it was responsible for substantial reductions in stem density in the midmarsh zone. In the Spartina patens dominated zone (high marsh), oil

and oil plus dispersant had the most impact while the dispersant had no effect.

Flowering shoot density was extremely low and patchy in the greenhouse experiment because of the small size of the microcosms. Consequently, these results have been eliminated from the analysis.

Biomass - The null hypotheses tested in this experiment were:

- H₁: Oil has no effect on total above-ground biomass production.
- H₂: Dispersant has no effect on total above-ground biomass production.
- H₃: There is no interaction (synergistic or inhibitory) between the factors oil and dispersant, affecting total above-ground biomass production.

These hypotheses were tested in each of the three vegetation zones. Results of the Student-Newman-Keuls multiple range tests and two-way analysis of variance procedures used to test these hypotheses are summarized in Appendix 9.

Figure 20 and Appendix 9 presents the results of the above-ground biomass harvest for the greenhouse experiment. The midmarsh zone was severely impacted by all of the oil and/or dispersant treatments. All treatments caused significant reductions ($\alpha = 0.05$, $df=4$) in above-ground biomass. Average above-ground living biomass in the oil, dispersant and oil plus dispersant treated microcosms were 13, 2, and 4% of the average for the control microcosms. The null hypotheses H₁ and H₂ were rejected because of these results. In the creek edge zone treatment impacts were intermediate in intensity. Average above-ground biomass for oiled microcosms was 85% of the control (not significantly different) while average above-ground biomass in the dispersant and oil plus dispersant treated microcosms were only 12 and 29% of average control biomass (significantly lower than the control microcosms ($\alpha = 0.05$, $df=4$)). The null hypothesis H₁ was retained and H₂ was rejected based on these results. The high marsh zone was impacted the least by the oil and/or dispersant treatments. Average above-ground biomass for oil, dispersant and oil plus dispersant treated microcosms were 76, 65 and 63% of the control values. All three treatments were significantly lower than the control. Although the high marsh zone was affected the least, the fact that these relatively small reductions were significant forced the rejection of the null hypotheses H₁ and H₂. No significant interactions were noted between oil and dispersant in the creek edge or high marsh zones, therefore the null hypothesis H₃ was retained in each zone. There was a significant interaction ($\alpha = 0.01$, $df=4$) in the

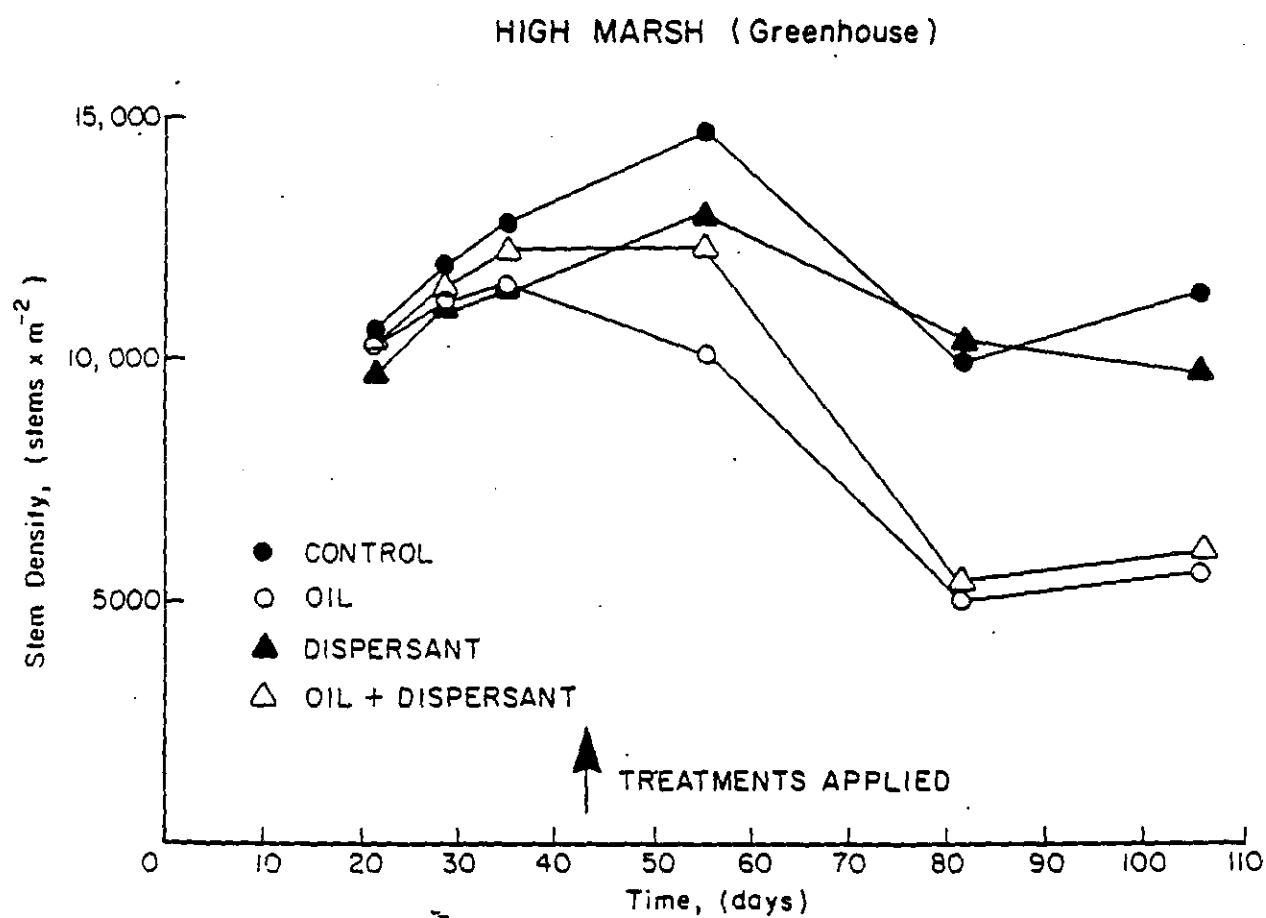


Figure 29: Mean stem density of *Spartina alterniflora* versus time. Data from control, oil and/or dispersant treated high marsh zone greenhouse microcosms are presented. Treatments were applied on day 43.

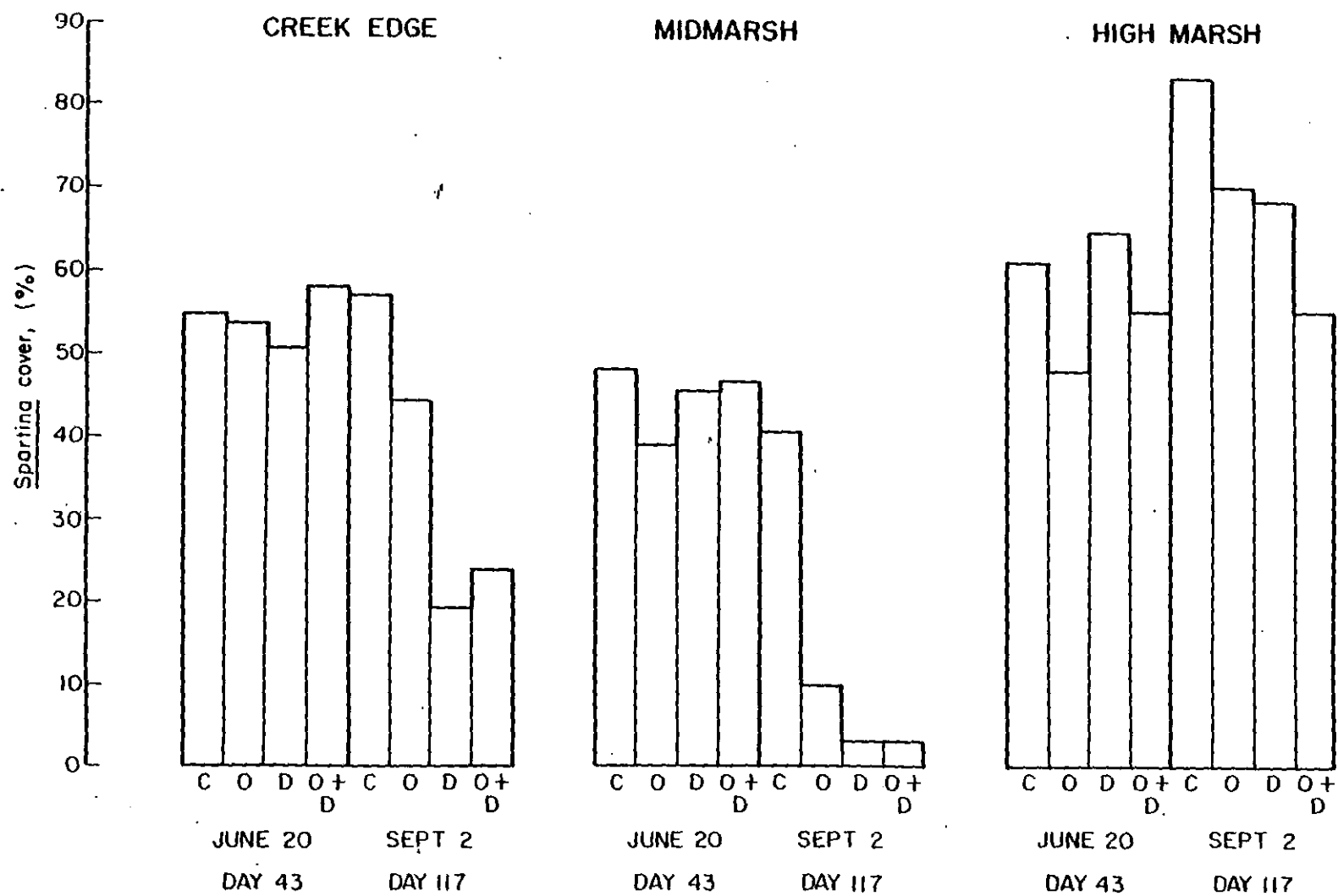


Figure 30: Histogram of mean *Spartina alterniflora* or *S. patens* cover in oil and/or dispersant treated creek edge, midmarsh, and high marsh zone microcosms. Data are from the greenhouse study.

midmarsh zone which caused the null hypothesis H_3 to be rejected. Oil appeared to inhibit the toxicity of the dispersant, possibly by forming a layer over the surface of the plant which excluded the more toxic dispersant.

Flowering plants were rare in the greenhouse experiment because of the relatively small size of the microcosms. These data are not included in the data analysis.

Species Cover - The null hypotheses tested in this experiment were:

- H_1 : Oil has no effect on Spartina alterniflora or S. patens cover.
- H_2 : Dispersant has no effect on Spartina alterniflora or S. patens cover.
- H_3 : There is no interaction (synergistic or inhibitory) between the factors oil and dispersant, affecting Spartina alterniflora or S. patens cover.

These hypotheses were tested in each of the three vegetation zones. Results of the Student-Newman-Keuls multiple range tests and two-way analysis of variance procedures used to test these hypotheses are summarized in Appendix 10.

Figure 30 illustrates the impacts of the oil and/or dispersant treatments on the percent cover of Spartina alterniflora or S. patens in the creek edge, midmarsh, and high marsh zones. S. alterniflora cover in the creek edge zone was reduced significantly ($\alpha = 0.05$, $df=4$) by all oil and/or dispersant treatments. S. alterniflora cover values in the dispersant and oil plus dispersant treated microcosms were most reduced while S. alterniflora cover in the oiled microcosms was least reduced. The null hypotheses, H_1 and H_2 were rejected on the basis of these results. There was a significant inhibition ($\alpha = 0.05$, $df=4$) of dispersant toxicity in the oil plus dispersant treated microcosms forcing the rejection of the null hypothesis H_3 . The oil film coating the plants may have neutralized the more toxic dispersant as it contacted the plant.

In the midmarsh all oil and/or dispersant treatments induced significant reductions ($\alpha = 0.05$, $df=4$) in S. alterniflora cover. The dispersant and oil plus dispersant treated microcosms were affected equally and most severely while the oiled microcosms suffered somewhat smaller reductions. The null hypotheses H_1 and H_2 were therefore rejected. As was noted in the creek edge zone, there was a significant inhibition ($\alpha = 0.01$, $df=4$) of dispersant toxicity by the oil in the oil plus dispersant treatment, causing the null hypothesis H_3 to be rejected.

Table 9 - Species composition of control, oil and/or dispersant treated greenhouse microcosms in the creek edge zone on two sampling dates. Treatments were applied on day 43.

Species	Treatment	Average Cover (1 Standard Deviation)	
		June 20 Day 43	Sept. 2 Day 117
<u>Spartina alterniflora</u>	Control	55.0(17.3)	57.0(5.7)
	Oil	54.0(12.4)	44.0(9.6)
	Dispersant	51.0(11.9)	19.0(4.2)
	Oil+Dispersant	58.0(9.1)	24.0(13.9)
<u>Salicornia europaea</u>	Control	1.1(0.9)	0.3(0.4)
	Oil	1.7(2.4)	0
	Dispersant	1.2(1.3)	0
	Oil+Dispersant	0.4(0.2)	0
<u>Atriplex patula</u>	Control	0	0
	Oil	0	0
	Dispersant	0.5(0.9)	0
	Oil+Dispersant	0.4(0.4)	0

In the high marsh zone, S. patens cover was slightly though not significantly reduced by all of the oil and/or dispersant treatments. Cover in the oil plus dispersant treated microcosms was decreased the most, while oil and dispersant treated microcosms experienced similar reductions. No significant interactions were noted between the oil and dispersant. These results justified the retention of the null hypotheses H_1 , H_2 and H_3 .

In summary, Spartina alterniflora cover was most affected by the oil and/or dispersant treatments in the midmarsh zone and least affected in the high marsh zone. Responses to the treatments were similar in the creek edge and midmarsh zones, with oil having the least impact and dispersant and oil plus dispersant treatments having greater impacts. Dispersant and oil plus dispersant treatments resulted in similar reductions in S. alterniflora cover. The oil significantly inhibited the toxicity of the dispersant in both the creek edge and midmarsh zones. In the high marsh zone, oil and dispersant caused similar reductions in Spartina patens cover with oil plus dispersant treatments causing somewhat greater mortality, however, these reductions were insignificant.

Species other than Spartina alterniflora and S. patens generally composed less than 3% of total cover in any of the vegetation zones (Tables 9-11). Trends in abundance were noted for several of these species. Salicornia europaea declined in cover in all vegetation zones and under all treatment regimes. This species is an annual and had probably reached the end of the vegetative phase of its life cycle by day 117 (September 2), thereby accounting for its reduction in salicornia europaea cover in all treatments. The same trend was noted for another annual, Atriplex patula, however, this species was not found in any of the control pots so it is impossible to determine whether this trend was caused by oil and/or dispersant toxicity or by natural senescence.

Leaf Anatomy - The microscopic symptoms observed in the greenhouse experiment were identical to those of the field experiment.

Fluorometry - The null hypotheses tested in this experiment were:

- H_1 : Oil has no effect on peak variable fluorescence of Spartina alterniflora or S. patens.
- H_2 : Dispersant has no effect on peak variable fluorescence of Spartina alterniflora or S. patens
- H_3 : There is no interaction (synergistic or inhibitory) between the factors oil and dispersant, affecting peak variable fluorescence of Spartina alterniflora or S. patens.

Table 11 - Species composition of control, oil and/or dispersant treated greenhouse microcosms in the high marsh zone on two sampling dates. Treatments were applied on day 43.

Species	Treatment	Average Cover (1 Standard Deviation)	
		June 20 Day 43	Sept. 2 Day 117
<u>Spartina patens</u>	Control	62.0(9.1)	84.0(4.2)
	Oil	48.0(11.5)	71.0(2.2)
	Dispersant	66.0(10.8)	69.0(16.7)
	Oil+Dispersant	56.0(16.0)	56.2(28.1)
<u>Spartina alterniflora</u>	Control	0.2(0.4)	0
	Oil	0	0
	Dispersant	0	0
	Oil+Dispersant	0.2(0.4)	0
<u>Salicornia europaea</u>	Control	0.2(0.4)	0
	Oil	0	0
	Dispersant	1.0(1.7)	0
	Oil+Dispersant	0.2(0.4)	0
<u>Plantago juncooides</u>	Control	3.4(6.5)	7.0(11.0)
	Oil	9.4(19.9)	0.1(0.2)
	Dispersant	2.4(4.3)	4.0(8.9)
	Oil+Dispersant	0	0
<u>Triglochin elata</u>	Control	1.0(2.2)	0
	Oil	0	0
	Dispersant	0	0
	Oil+Dispersant	0	0
<u>Atriplex patula</u>	Control	0	0
	Oil	0	0
	Dispersant	0.4(0.9)	0
	Oil+Dispersant	0	0

Table 10 - Species composition of control, oil and/or dispersant treated greenhouse microcosms in the midmarsh zone on two sampling dates. Treatments were applied on day 43.

Species	Treatment	Average Cover (1 Standard Deviation)	
		June 20 Day 43	Sept. 2 Day 117
<u>Spartina alterniflora</u>	Control	48.6(11.6)	41.0(4.2)
	Oil	39.4(7.2)	10.0(3.5)
	Dispersant	46.4(12.6)	3.0(4.5)
	Oil+Dispersant	47.0(2.7)	3.0(4.5)
<u>Spartina patens</u>	Control	0	0.4(0.9)
	Oil	0	0
	Dispersant	0	0
	Oil+Dispersant	0	0
<u>Salicornia europaea</u>	Control	0.3(0.4)	0
	Oil	0	0
	Dispersant	0.1(0.2)	0
	Oil+Dispersant	0	0
<u>Plantago juncooides</u>	Control	1.0(2.2)	0
	Oil	0	0
	Dispersant	0	0
	Oil+Dispersant	0	0

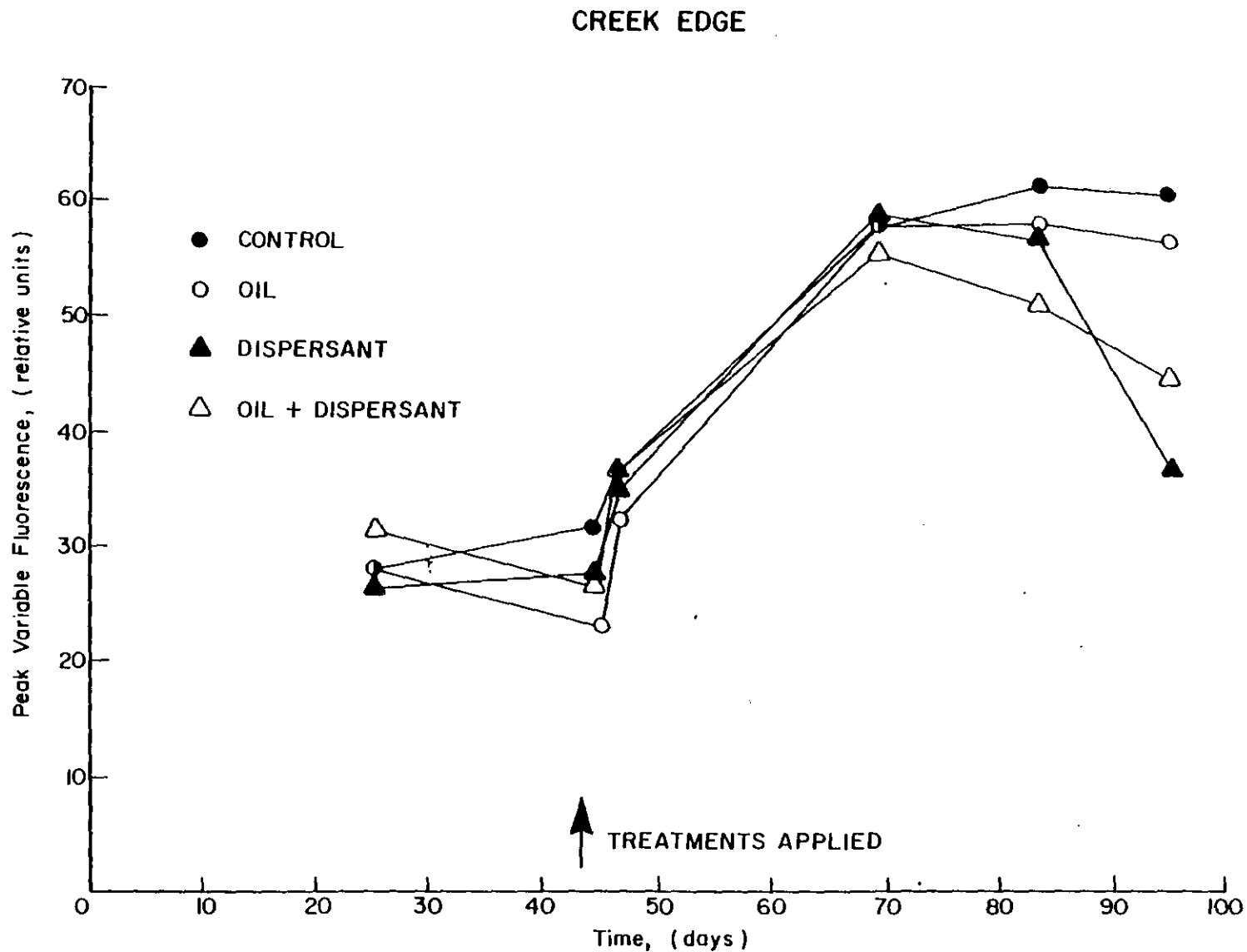


Figure 31: Plot of peak variable fluorescence values versus time for control, oil and/or dispersant treated plants from the creek edge microcosms on six sampling dates. Treatments were applied on day 43.

- H₄: Oil has no effect on 100 second difference values of Spartina alterniflora or S. patens.
- H₅: Dispersant has no effect on 100 second difference values of Spartina alterniflora or S. patens.
- H₆: There is no interaction (synergistic or inhibitory) between the factors, oil and dispersant affecting 100 second differences values of Spartina alterniflora or S. patens.

These hypotheses were tested in each of the three vegetation zones. Results of the Student-Newman-Keuls multiple range tests and two-way analysis of variance procedures used to test these hypotheses are summarized in Appendix 11.

Peak variable fluorescence values for the four treatments are plotted for the creek edge, midmarsh and high marsh zones in Figures 31-33. Control treatments for all of the zones exhibited an increase in peak fluorescence through the growing season, although peak fluorescence for the two Spartina alterniflora zones (creek edge and midmarsh) began to level off between day 69 (middle of July) and day 95 (middle of August).

In the creek edge zone, values for oil treated quadrats were not significantly different from control values throughout the experiment (Figure 31, Appendix 11), except on the day after the treatments were applied (day 43) when they were significantly lower ($\alpha = 0.05$, $df=4$) than the control. The dispersant and oil plus dispersant treatments deviated significantly ($\alpha = 0.05$, $df=4$) from the control only on the last day of observation. The Student-Newman-Keuls procedure indicated that all treatments except the control and oil treatments were significantly different from each other on day 96. Based on these results, the null hypothesis H₁ was retained. The null hypothesis H₂, however, was rejected. Although significant reductions were noted on only one day, trends in the data suggest a gradual reduction of peak variable fluorescence for the dispersant and oil plus dispersant treatments. Two way analysis of variance results indicated a significant inhibitory interaction ($\alpha = 0.05$, $df=4$) between oil and dispersant on day 44, however, this interaction did not persist. The null hypothesis H₃ was therefore retained.

In the midmarsh zone, peak fluorescence values for the oil and/or dispersant treatments (Figure 32, Appendix 11) were generally much lower than control values with the exceptions of the dispersant treatment on day 69 and the oil treatment on day 83. The data, however, were highly variable resulting in inconsistencies in the trends for the statistics. Student-Newman-Keuls procedures indicated that peak fluorescence values for the oil treated pots were significantly higher than those of the oil plus

HIGH MARSH

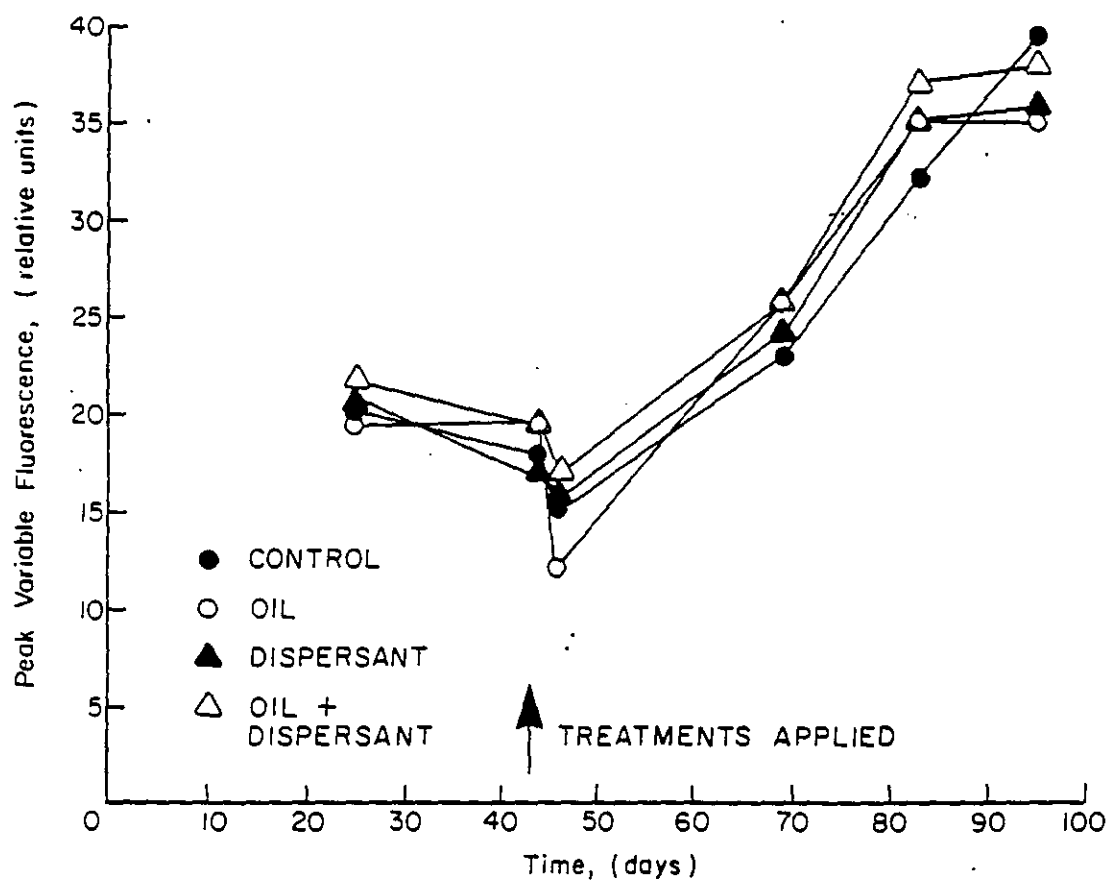


Figure 33: Plot of peak variable fluorescence values versus time for control, oil and/or dispersant treated plants from the high marsh microcosms on six sampling dates. Treatments were applied on day 43.

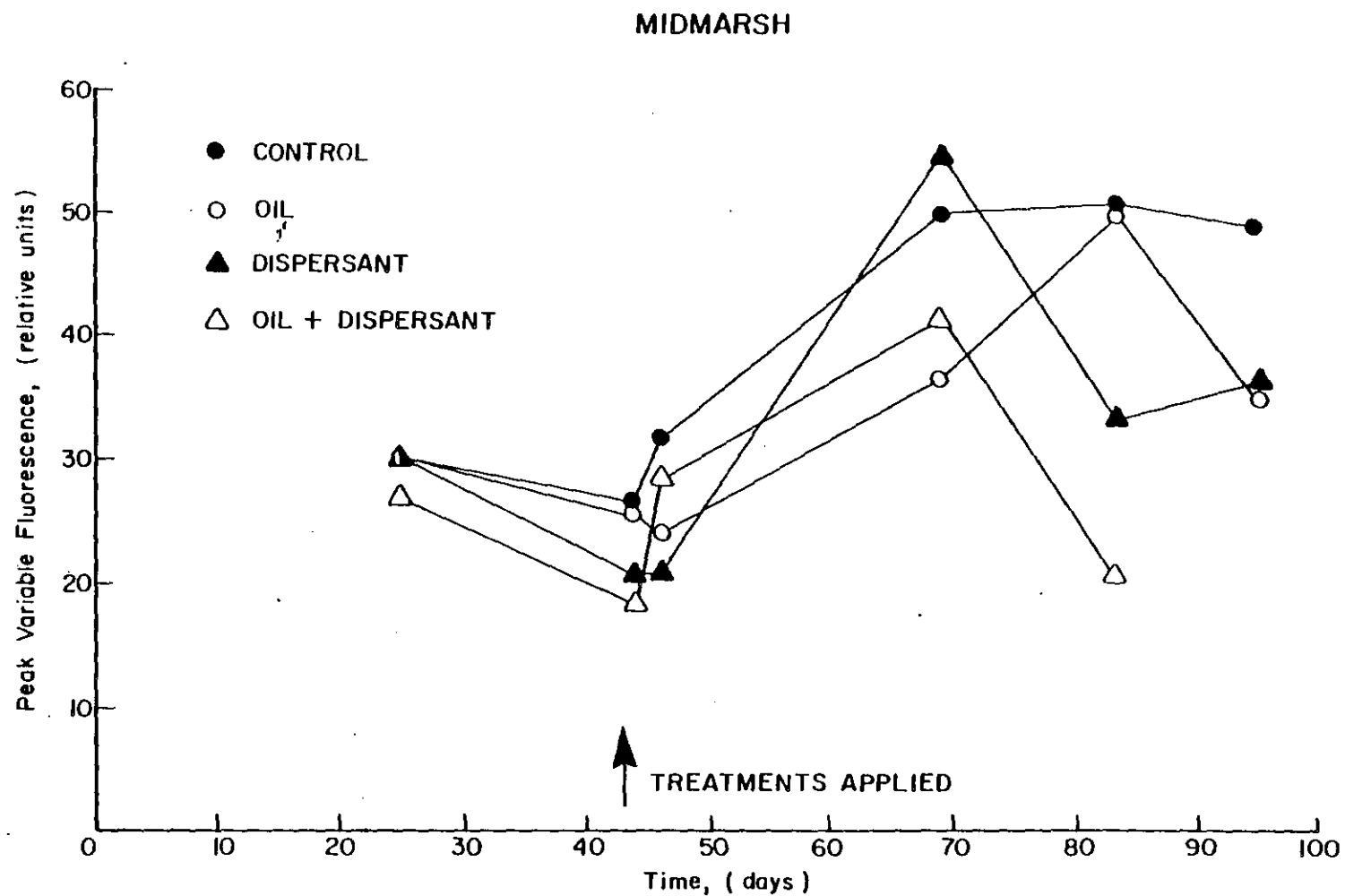


Figure 32: Plot of peak variable fluorescence values versus time for control, oil and/or dispersant treated plants from the midmarsh microcosms on six sampling dates. Treatments were applied on day 43. Data for oil plus dispersant treated plants were unavailable on day 95.

CREEK EDGE

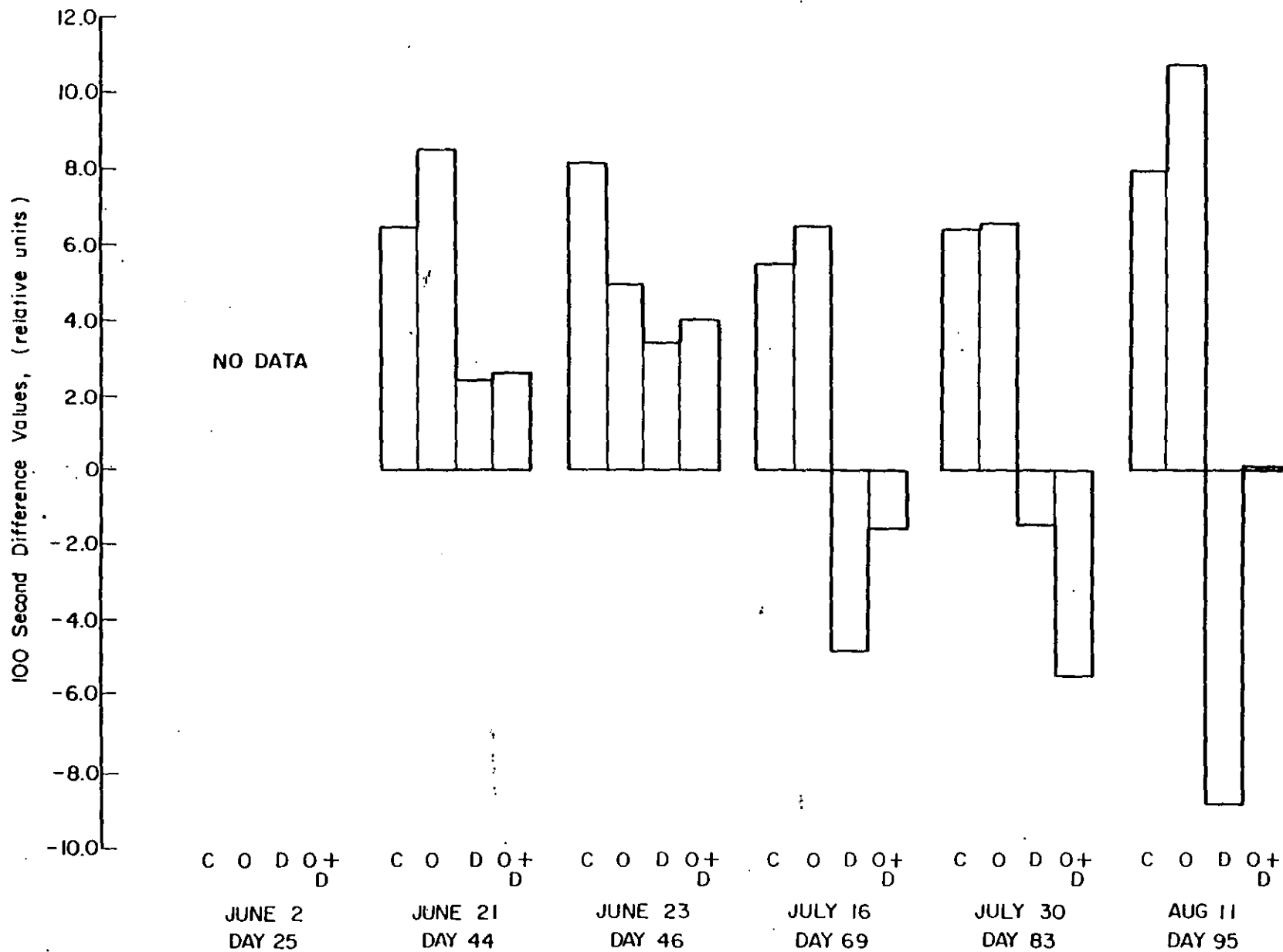


Figure 34: Histogram of one hundred second difference values for control, oil and/or dispersant treated plants from the creek edge microcosms on five sampling dates. Treatments were applied on day 43. Pretreatment data were unavailable on day 25.

dispersant treated pots on day 44 ($\alpha = 0.05$, $df=4$) while on day 46 the only significant difference was between the control and dispersant pots ($\alpha = 0.05$, $df=4$). On day 69, peak fluorescence values for the oiled pots were significantly lower ($\alpha = 0.05$, $df=4$) than both the control and dispersant pots. Peak variable fluorescence for the oil plus dispersant treated pots was significantly lower ($\alpha = 0.05$, $df=4$) than those of all other treatments on day 83. Lack of data for the oil plus dispersant treated plots prevented statistical analysis of the fluorometry results of day 95. Although significant reductions in peak variable fluorescence associated with the various treatments were not consistent, the overall trend suggests that the treatments produced a considerable impact. Therefore the null hypotheses H_1 and H_2 were rejected. Results of the two-way analysis of variance indicated a significant inhibitory interaction ($\alpha = 0.05$, $df=4$) between oil and dispersant on day 46 which did not persist. The lack of persistence of the interaction suggested that it may have been an anomaly possibly caused by sampling problems. The null hypothesis H_3 was therefore retained.

The unusually high peak fluorescence readings for the dispersant treated microcosms on day 69 and the oil treated microcosms on day 83 correspond to periods following large reductions in stem density. Surviving plants were generally concentrated around the edges of the microcosms where exposure to the oil or dispersant was lowest. Selection of leaves for analyses was restricted to these relatively healthy plants, consequently, an increase in peak fluorescence might be expected at this time. The decline in peak fluorescence following these peaks corresponded to a gradual deterioration of the condition of these surviving plants.

In the high marsh zone, peak fluorescence values for the oil and/or dispersant treatments were not significantly different from those of the control (Figure 33, Appendix 11) at any time. No significant interactions between oil and dispersant were noted. Based on these results, the null hypotheses H_1 , H_2 and H_3 were retained.

One hundred second difference values for the four treatments are plotted against time for the creek edge, midmarsh, and high marsh zones (Figures 34-36). In the creek edge zone, (Figure 34, Appendix 11), 100 second difference values for oil treated microcosms were not significantly different from control values on any sampling date. One hundred second difference values for dispersant and oil plus dispersant treatments declined within one day of treatment applications and remained low, however, these reductions were statistically significant ($\alpha = 0.05$, $df=4$) only on days 69, 83 and 95. These results indicated that the null hypothesis H_4 was true while H_5 was false. No significant interactions

MIDMARSH

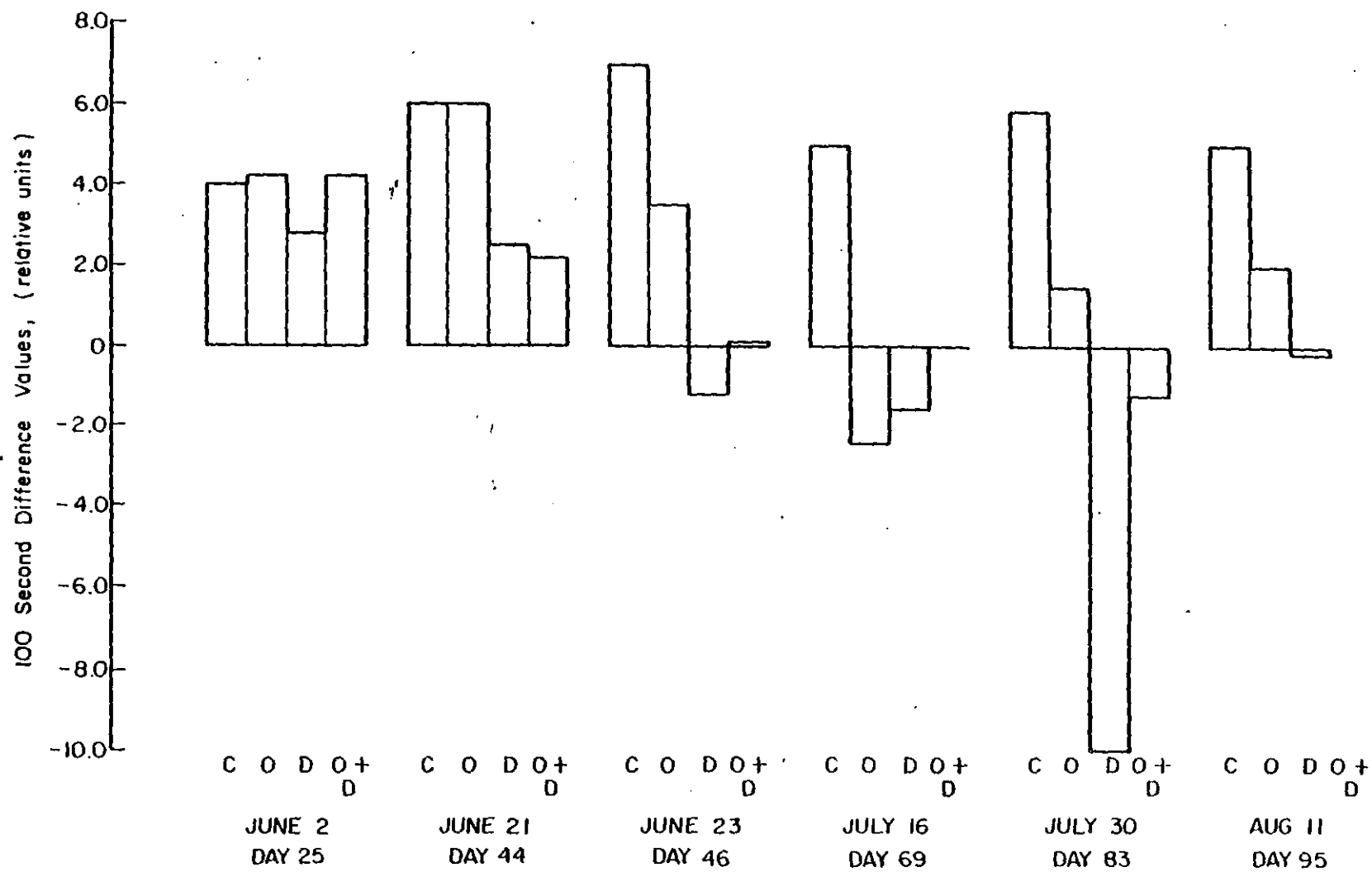


Figure 35: Histogram of one hundred second difference values for control, oil and/or dispersant treated plants from the midmarsh microcosms on five sampling dates. Treatments were applied on day 43.

were noted between the oil and dispersant suggesting that the null hypothesis H_6 was true.

In the midmarsh zone (Figure 35, Appendix 11), all oil and/or dispersant treatments resulted in declines in 100 second difference values. Values for the dispersant and oil plus dispersant treated plants declined within one day of treatment application (day 44). Two way analysis of variance results indicated a significant reduction ($\alpha = 0.01$, $df=4$) in 100 second difference values for the dispersant treatment on this day, however, multiple range tests indicated no significant reductions. Oil treated plants exhibited no reductions in 100 second difference values at this time. On day 46, values for all treatments were reduced relative to the control, however, only the dispersant and oil plus dispersant treated plants exhibited significant reductions ($\alpha = 0.05$, $df=4$). By day 69, all oil and/or dispersant treatments had significantly reduced ($\alpha = 0.05$, $df=4$) 100 second difference values. Based on these results, the null hypotheses H_4 and H_5 were rejected. Analysis of variance results for day 69 indicated a significant inhibitory interaction ($\alpha = 0.01$, $df=4$) between oil and dispersant which was also evident on day 83. The null hypothesis H_6 was rejected as a consequence of these results.

One hundred second difference values for oil treated microcosms had begun to recover by day 83 and were no longer significantly different from the control, while the dispersant and oil plus dispersant treated plants still had significantly reduced values ($\alpha = 0.05$, $df=4$). Data for day 95 indicated continued recovery of 100 second difference values for oil treated plants and the beginning of a recovery phase for dispersant treated plants. Statistical analyses were not performed on the data for this day since data for oil plus dispersant treated plants were unavailable. The onset of recovery phases was associated with a large reduction in stem density. Selection of plants was thus restricted to relatively healthy plants concentrated around the edges of the microcosms. This suggests that the apparent recovery of oil and dispersant treated plants is erroneous.

One hundred second difference values for the high marsh zone were relatively unaffected by the treatments (Figure 36, Appendix 11). Values for dispersant and oil plus dispersant treatments were significantly reduced ($\alpha = 0.05$, $df=4$) one day after treatment applications but recovered to control levels within three days. On day 46 oil and dispersant treatments had 100 second difference values higher than the control, and on day 69, dispersant and oil plus dispersant treated plants had elevated values, however, these fluctuations were not statistically significant. On days 83 and 106, there were no differences between treatments. The null hypotheses H_4 and H_5 were therefore retained. No significant interactions

were noted between the oil and dispersant treatments; therefore, the null hypothesis H_0 was also retained.

Fluorescence induction measurements provide evidence of damage to the photosynthetic systems of Spartina alterniflora and S. patens in all three vegetation zones. Plant stress was apparent within one day of treatment applications, two days before any visible signs of stress were noticed. Fluorescence effects varied between treatments within zones in the same way that other symptoms varied, providing a useful forecasting method for other symptoms. Damage to photosynthetic systems was heaviest in the midmarsh zone and lightest in the high marsh zones. In the creek edge and midmarsh zones, plant stress lasted for the duration of the experiment while in the high marsh zone it was temporary. These data suggest that S. patens is more tolerant of oil and/or dispersant than S. alterniflora.

Dispersant alone was the most toxic treatment in all vegetation zones. The oil plus dispersant treatments generally induced less stress than the dispersant treatment. Oil was applied to these plants before the dispersant and may have acted as a barrier to entry of the dispersant into plant tissues, however, this inhibitory interaction was significant only in the midmarsh microcosms. Oil treatments caused relatively little stress in all but the midmarsh zone.

The 100 second difference values were the most sensitive measure of toxic effects, indicating plant stress within one day of treatment application. Peak fluorescence was much less sensitive, reliably indicating stress only near the time of death of the plants when visible symptoms were apparent.

The fluorescence effects observed consisted of reduced peak fluorescence and a faster return to initial values. These are suggestive of damage to the light receptor system.

Comparison of Vascular Plant Effects Noted in the Field and Greenhouse Experiments

The results of the field and greenhouse experiments involving vascular plants were generally very similar. Table 12 summarizes the results of each of the field and greenhouse experiments conducted on the vascular plants. Treatments are ranked in increasing magnitude of the parameter studied and the greater than, less than, or equal to signs indicate the relationships between treatments. There were only minor differences between field and greenhouse results for the plant height, Spartina cover and leaf anatomy experiments. The largest differences between field and greenhouse results were found in the stem density experiment. The

HIGH MARSH

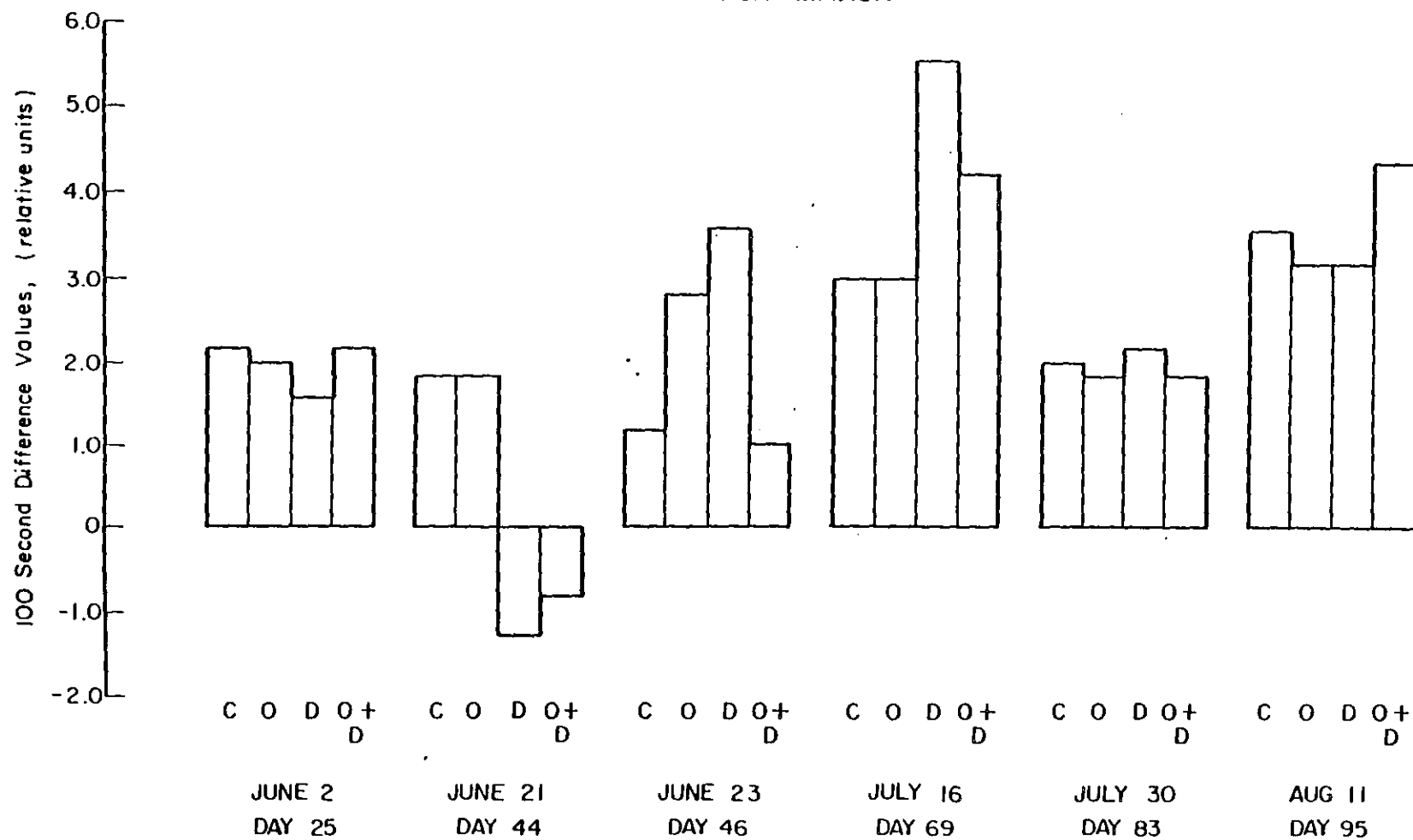


Figure 36: Histogram of one hundred second difference values for control, oil and/or dispersant treated plants from the high marsh microcosms on six sampling dates. Treatments were applied on day 43.

greenhouse experiment indicated that the creek edge and high marsh zones were equally susceptible to the oil and/or dispersant treatments while the field results agreed with all other parameters by indicating that the high marsh zone was the least susceptible. Field and greenhouse results for the high marsh zone indicated that oil was marginally more toxic than oil + dispersant and that the dispersant had the least impact. Field and greenhouse results differed somewhat in that dispersant was relatively more toxic in the field experiment than in the greenhouse experiment. Larger differences were noted between the field and greenhouse results for the creek edge zone. Large reductions in stem density were associated with the oil plus dispersant treatments in the field, however, reductions associated with oil plus dispersant treatments in the greenhouse were insignificant. A major reason for the relatively poor correspondence between the creek edge field and greenhouse results was the high variability of stem density between plots or microcosms which may have masked treatment related trends and altered the interaction of the treatments with the plants.

Stem density was consistently higher in the green house study than in the field. Greenhouse creek edge, midmarsh, and high marsh stem density values for controls were on average 1.7, 1.5 and 4.0 times higher than those in the field study. In the greenhouse experiment detritus was removed for use in another experiment. This decreased the shading of the sediment surface (particularly in the high marsh zone which maintains a very large detrital standing crop) and increased the survival rates of new tillers or stimulated their production.

Above-ground biomass values for various treatments in the creek and midmarsh vegetation zones were very similar, however, substantial differences were noted between the field and greenhouse experiments in the high marsh zone. Above-ground biomass was consistently higher in the greenhouse experiment than in the field experiment. This is attributable to the stimulation of tiller production associated with the removal of detritus in the green house high marsh microcosms.

In the field experiment only the dispersant treatment caused significant reductions in biomass, while in the field experiment, all treatments caused a small but significant reduction in biomass. Relative reductions of above-ground biomass were actually higher in the field experiment than in the greenhouse experiment. In the field experiment treated plots produced between 39 and 70% of control biomass, while in the greenhouse experiment, they produced between 63 and 76%. The biomass reductions in the greenhouse study were significant because of the unusually low variances associated with the means of the various treatments.

Table 12 - Summary table comparing results from the field and greenhouse experiments on vascular plants. Treatments and zones are arranged in descending order of impact. Signs indicate impact relationships between treatments or zones. C=creek edge; M=midmarsh; H=high marsh; C=control; O=oil; D=dispersant; O+D=oil+dispersant.

		Field Experiment		Greenhouse Experiment	
Parameter	Zone	Order of Impact	Overall Impact	Order of Impact	Overall Impact
Plant Height	C	D>O+D>O=C	M>C>H	D>O+D>O=C	M>C>H
	M	D>O+D>O=C		D>O+D>O=C	
	H	D=O+D=O=C		D=O+D=O=C	
Plant Density	C	O+D>D=O>C	M>C>H	O+D=D>O=C	M>H=C
	M	O+D>D=O>C		O+D=D=O>C	
	H	O>O+D=D>C		O=O+D>D=C	
Aboveground Biomass	C	O+D=D>O=C	M>C>H	O+D=D>O=C	M>C>H
	M	O+D=D>O>C		O+D=D=O>C	
	H	D=O+D=O=C		O+D=D=O>C	
<u>Spartina</u> Cover	C	O+D>D>O>C	M>C>H	D=O+D>O>C	M>C>H
	M	O+D=D=O>C		O+D=D=O>C	
	H	O=D=O+D>C		O+D=D=O>C	
Leaf Anatomy	Symptoms similar in all zones and between all oil and/or dispersant treatments.			Symptoms similar in all zones and between all oil and/or dispersant treatments.	

Table 13 - Algal and bacterial cover in the creek edge field plots on two sampling dates. Treatments were applied on day 60.

Treatment	Algal and Bacterial Types	Average Cover (1 Standard Deviation)	
		July 22 Day 75	Sept. 10 Day 125
Control	Algal Mat	0.03(0.06)	0.3(0.6)
	Sulfur-Oxidizing Bacteria	1.7(2.9)	0.7(1.2)
	Sulfate-Reducing Bacteria	0	1.0(1.7)
Oil	Algal Mat	1.3(2.2)	0
	Sulfur-Oxidizing Bacteria	17.1(14.6)	3.3(5.8)
Dispersant	Algal Mat	2.3(3.9)	36.7(30.6)
Oil plus	Algal Mat	3.9(4.3)	5.3(4.5)
Dispersant	Sulfur-Oxidizing Bacteria	1.3(2.2)	0

Impacts on Microbial Populations

Field Experiment

Algal and Bacterial Cover - Statistical analysis was restricted to the algal mat component since it was generally the most abundant microbial type. The null hypotheses to be tested were:

- H_1 : Oil has no effect on algal cover.
- H_2 : Dispersant has no effect on algal cover.
- H_3 : There is no interaction (synergistic or inhibitory) between the factors, oil and dispersant affecting algal cover.

These hypotheses were tested in each of the three vegetation zones. The results of the Student-Newman-Keuls multiple range tests and the two-way analyses of variance procedures are summarized in Appendix 12.

The distributions of the four microbial zones noted in the field study were very patchy which made interpretation of the data difficult. In the creek edge zone (Table 13, Appendix 12), algal mat, sulfur-oxidizing bacteria and sulfate reducing bacteria communities were noted, however, only the first two types occurred consistently. Between days 75 and 125 (15-65 days after treatment application), algal cover remained relatively stable in the control and oil plus dispersant treatments, increased greatly in the dispersant treated plots and disappeared in the oiled plots. On day 75 there was no significant difference between algal cover on any of the treatments, however, on day 125 algal cover on the oiled plots was significantly lower ($\alpha = 0.05$, $df=2$) than the control while algal cover in the dispersant and oil plus dispersant treated plots was significantly higher ($\alpha = 0.05$, $df=2$) than the control, suggesting that the oil treatment was relatively more toxic than the dispersant treatment. No significant interactions were noted between the oil and dispersant treatments; therefore, the null hypothesis H_3 was retained. Based on these results, the null hypothesis H_1 was rejected. Although a significant change in algal mat cover was associated with the dispersant treatment (consequently H_2 must be rejected), it is unclear whether this was directly or indirectly attributable to the dispersant. The increase in algal cover in the dispersant treated plots was probably caused by the increased availability of light and nutrients associated with the decline of the macrophyte over story.

Sulfur-oxidizing bacteria declined between days 75 and 125 in all treatments suggesting that normal seasonal abundance patterns were not affected by the treatments. The high cover value for the oil treated plots

Table 14 - Algal and bacterial cover in the midmarsh field plots on two sampling dates. Treatments were applied on day 60.

Treatment	Algal and Bacterial Types	Average Cover (1 Standard Deviation)	
		July 22 Day 75	Sept. 10 Day 125
Control	Algal Mat	29.2(33.8)	17.3(28.3)
	Sulfur-Oxidizing Bacteria	1.7(2.9)	0
Oil	Algal Mat	12.1(11.3)	18.3(2.9)
	Sulfur-Oxidizing Bacteria	20.4(32.2)	0
	Sulfate-Reducing Bacteria	0	1.7(2.9)
Dispersant	Algal Mat	1.7(2.9)	2.0(2.6)
	Sulfur-Oxidizing Bacteria	2.9(5.1)	0
Oil plus	Algal Mat	8.0(1.4)	45.0(8.7)
Dispersant	Sulfur-Oxidizing Bacteria	25.0(10.9)	0
	Photosynthetic Bacteria	1.7(2.9)	0

on day 75 suggests that the oil may have directly (through direct contribution of sulfur compounds) or indirectly (through alteration of microbial metabolism), provided a source of hydrogen sulfide.

In the midmarsh zone (Table 14, Appendix 12), all four microbial types were observed, however, only the algal mat and sulfur-oxidizing bacteria were found consistently. The algal cover in the control, oil, and dispersant plots remained relatively stable over time. Algal mat cover in the oil plus dispersant treated plots increased between days 75 and 125. There was no significant difference between the algal covers of any of the treatments on day 75 or 125. These data suggest that the algal mat in the midmarsh zone was relatively tolerant of all treatments. Therefore, the null hypotheses H_1 and H_2 were retained. The increase in algal mat cover in the oil plus dispersant treated plots may have been caused by the increased availability of light and nutrients associated with the decline of the macrophyte over story. No significant interactions were noted between the oil and dispersant, therefore, the null hypothesis H_3 was retained.

The sulfur-oxidizing bacteria in the midmarsh zone responded in the same manner as that in the creek edge zone. The cover of this community declined in all treatments suggesting that normal seasonal patterns of abundance were not affected by the treatments, however, cover in the oil and oil plus dispersant treatments were unusually high suggesting direct or indirect stimulation of this community by the oil applications.

Only the algae and sulfur-oxidizing bacteria were found in the high marsh zone (Table 15, Appendix 12). Algal cover disappeared in the control and dispersant treated quadrats between days 75 and 125 while in the oiled plots, it increased. Although this increase was large, the oiled plots did not have significantly higher cover on day 125 as a result of the large variance associated with the cover in this treatment. No significant difference was found between treatments on day 75. The increase in algal cover may be related to the large reduction of stem density in the oiled plots which would make light and nutrients available to the algal mat. The results suggest that the treatments had little direct effect on algal cover in the high marsh zones, therefore, the null hypotheses H_1 and H_2 were retained. No significant interaction was noted between the oil and dispersant. Based on this result, the null hypothesis H_3 was retained.

The cover of sulfur-oxidizing bacteria in the high marsh zone responded to time and treatments in the same manner as it did in the creek edge and midmarsh zones.

Greenhouse Experiment

Algal and Bacterial Cover - As in the field experiment, statistical testing was conducted on only the algal cover data. the null hypotheses tested in this experiment were:

- H₁: Oil has no effect on algal cover.
- H₂: Dispersant has not effect on algal cover.
- H₃: There is no interaction (synergistic or inhibitory) between the factors, oil and dispersant affecting algal cover.

These hypotheses were tested on the creek edge, midmarsh and high marsh microcosms. The results of the Student-Newman-Keuls multiple range tests and two-way analysis of variance procedures used to test these hypotheses are summarized in Appendix 13.

In the creek edge zone (Table 16, Appendix 13), only algae and sulfate-reducing bacteria were noted. The algal cover of the control and oil plus dispersant treatments remained relatively stable from day 39 to day 106. In the oiled microcosms algal cover was reduced on day 67 (24 days after oiling) but returned to the pretreatment level by day 106. Algal cover in dispersant treated microcosms increased through the growing season. No significant differences in algal cover between treatments were found on days 39 and 67. On day 106, the dispersant treated plots had significantly higher ($\alpha = 0.05$, $df=4$) cover than the control. These results suggest that the oil and/or dispersant treatments had little direct effect on algal cover. The reduction in algal cover in the oiled plots was temporary and statistically insignificant, therefore, the null hypothesis H₁ was retained. The significant increase in algal mat cover associated with the dispersant treatment suggests that the null hypothesis H₂ should be rejected. It is not clear, however, whether or not the dispersant directly affected the algae. It is likely that the dispersant indirectly stimulated algal mat development through the destruction of the macrophyte canopy which would increase the availability of light and nutrients to the algae. Significant interactions between the oil and dispersant were not observed. The null hypothesis H₃ was therefore, retained.

Sulfate-reducing bacteria were not observed in the microcosms until day 106. Similar cover values were observed in the control, oil and/or dispersant treatments suggesting that these treatments had no impact on sulfate-reducing bacteria. The development of reducing zones late in the summer (day 106) would be expected since peak heterotrophic activity at this time would bring anaerobic zones closer to the surface.

Table 15 - Algal and bacterial cover in the high marsh field plots on two sampling dates. Treatments were applied on day 60.

Treatment	Algal and Bacterial Types	Average Cover (1 Standard Deviation)	
		July 22 Day 75	Sept. 10 Day 125
Control	Algal Mat	0.8(1.4)	0
	Sulfur-Oxidizing Bacteria	0.4(0.8)	0
Oil	Algal Mat	0.2(0.3)	25.0(35.0)
	Sulfur-Oxidizing Bacteria	3.9(5.3)	0
Dispersant	Algal Mat	1.2(1.3)	0
Oil plus Dispersant	Sulfur-Oxidizing Bacteria	0.03(0.06)	0

All four algal and/or bacterial types were observed in the midmarsh zone (Table 17, Appendix 13). The algal mat community was particularly well developed in this zone. In the control and dispersant treated microcosms algal cover remained relatively stable over time, while in the oil and oil plus dispersant treated microcosms it was significantly reduced ($\alpha = 0.05$, $df=4$) on day 67 but recovered to the control level by day 106. There were no significant differences between treatments on days 39 and 106. This evidence suggests that the oil treatments caused an acute reduction in algal cover (probably by smothering of the algae) which quickly dissipated. The null hypothesis H_1 was rejected. The null hypothesis H_2 , however was retained. No significant interactions were noted between the oil and dispersant, therefore, the null hypothesis H_3 was retained.

Cover values for sulfur reducing and sulfur oxidizing bacteria were reduced somewhat by the dispersant treatment and remained low through the experiment. Cover values in the other treatments, however, remained stable, suggesting that oil and dispersed oil had no affect on the sulfate reducing and sulfur oxidizing bacterial types.

Photosynthetic bacteria were found in all oil and/or dispersant treated microcosms following the application of treatments. Cover of this type peaked 24 days after treatment applications (day 67) then declined to low levels by day 106. Cover values were highest in the dispersant treated pots on day 67. Spartina cover was reduced by approximately 90% in these microcosms at this time, resulting in increased availability of light and nutrients to the pink photosynthetic bacteria. Expansion of the algal mat eventually replaced or obscured the photosynthetic bacteria.

Algae were the only microbial type observed in the high marsh zone (Table 18, Appendix 13). Algal cover in this zone generally increased over time in all treatments. In the oil and oil plus dispersant treated microcosms, a reduction in algal cover was noted; however, it was not statistically significant and cover values returned to the level of the control by day 106. No significant differences in algal cover between treatments were found on any of the sampling dates, nor were significant interactions noted between the treatments. Therefore, the null hypotheses H_1 , H_2 and H_3 were retained.

In summary, algal cover data for all three vegetation zones exhibited the same trend. Microcosms receiving oil or oil plus dispersant treatments experienced a decline in algal cover followed by a fairly rapid recovery. A similar trend was noted by Baker (1971b). The dispersant treatment

Table 16 - Algal and bacterial cover in the creek edge microcosms on three sample dates. Treatments were applied on day 43.

Treatment	Algal and Bacterial Types	Average Cover (1 Standard Deviation)		
		June 16 Day 39	July 14 Day 67	Aug. 22 Day 106
Control	Algal Mat	6(9)	15(21)	11(15)
	Sulfur-Oxidizing Bacteria	0	0	3(7)
Oil	Algal Mat	20(21)	2(3)	16(20)
	Sulfur-Oxidizing Bacteria	0	0	1(2)
Dispersant	Algal Mat	18(16)	26(34)	50(32)
	Sulfate-reducing Bacteria	0	0	2(3)
Oil plus Dispersant	Algal Mat	10(12)	8(13)	15(7)

Table 18 - Algal and bacterial cover in the high marsh microcosms on three sampling dates. Treatments were applied on day 43.

Treatment	Algal and Bacterial Types	Average Cover (1 Standard Deviation)		
		June 16 Day 39	July 14 Day 67	Aug. 22 Day 106
Control	Algal Mat	6(13)	8(18)	16(36)
Oil	Algal Mat	4(9)	1(2)	14(31)
Dispersant	Algal Mat	0	0	8(12)
Oil plus Dispersant	Algal Mat	2(5)	0	8(13)

Table 17 - Algal and bacterial cover in the midmarsh microcosms on three sampling dates. Treatments were applied on day 43.

Treatment	Algal and Bacterial Types	Average Cover (1 Standard Deviation)		
		June 16 Day 39	July 14 Day 67	Aug. 22 Day 106
Control	Algal Mat	96(6)	99(2)	94(6)
	Sulfate-Reducing Bacteria	2(5)	1(1)	4(6)
	Sulfur-Oxidizing Bacteria	2(5)	1(1)	2(3)
	Photosynthetic Bacteria	0	0	0
Oil	Algal Mat	88(18)	6(8)	69(44)
	Sulfate-Reducing Bacteria	10(17)	5(3)	8(15)
	Sulfur-Oxidizing Bacteria	2(5)	1(1)	4(6)
	Photosynthetic Bacteria	0	1(1)	1(1)
Dispersant	Algal Mat	78(18)	68(29)	99(1)
	Sulfate-Reducing Bacteria	10(7)	2(2)	1(1)
	Sulfur-Oxidizing Bacteria	12(13)	2(1)	1(1)
	Photosynthetic Bacteria	0	27(29)	0
Oil plus	Algal Mat	96(9)	11(14)	88(6)
Dispersant	Sulfate-Reducing Bacteria	2(5)	5(11)	5(6)
	Sulfur-Oxidizing Bacteria	2(5)	6(7)	6(5)
	Photosynthetic Bacteria	0	4(2)	1(1)

CREEK EDGE (Greenhouse)

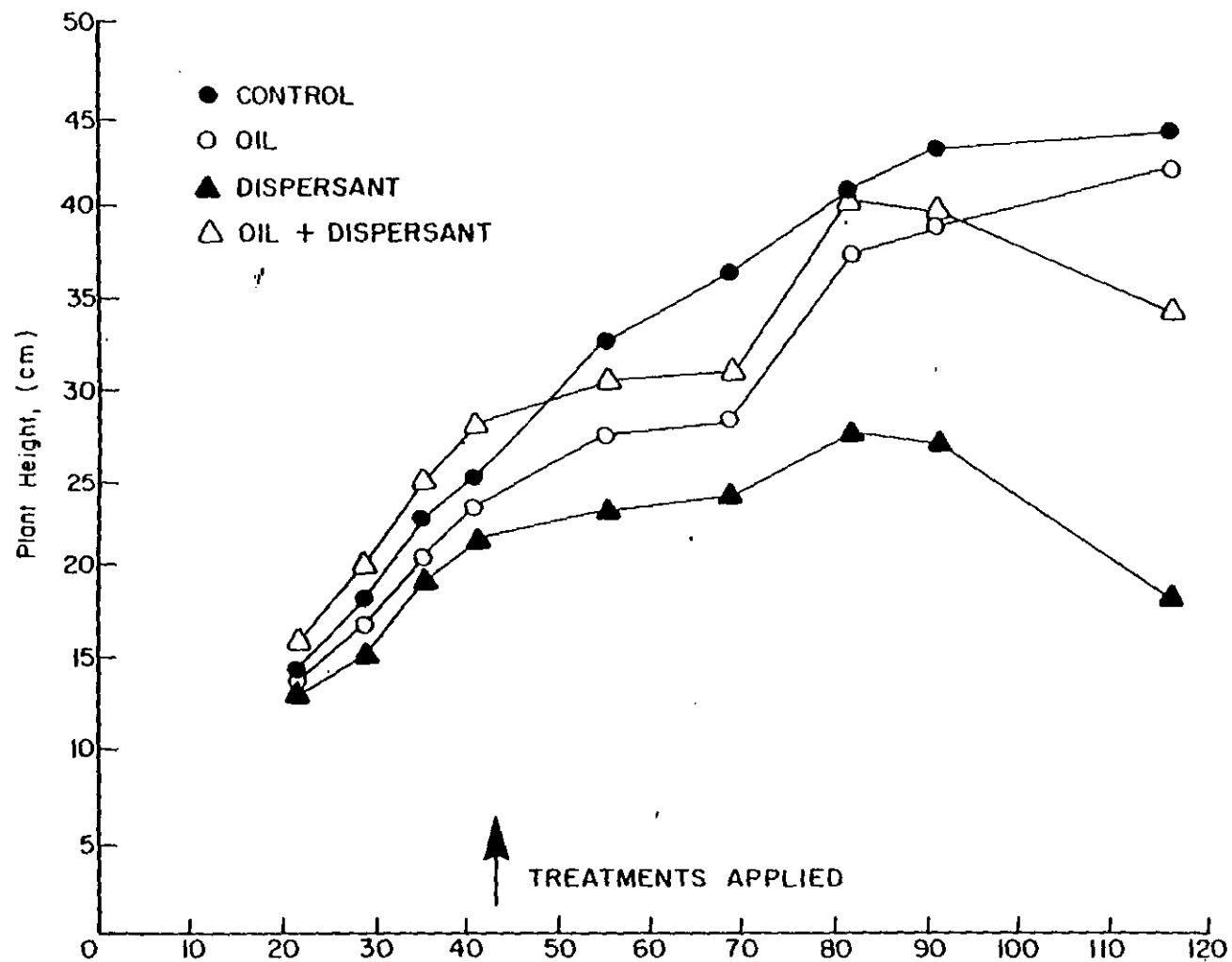


Figure 21: Mean *Spartina alterniflora* shoot height for the creek edge zone versus time. Data are from the greenhouse study.

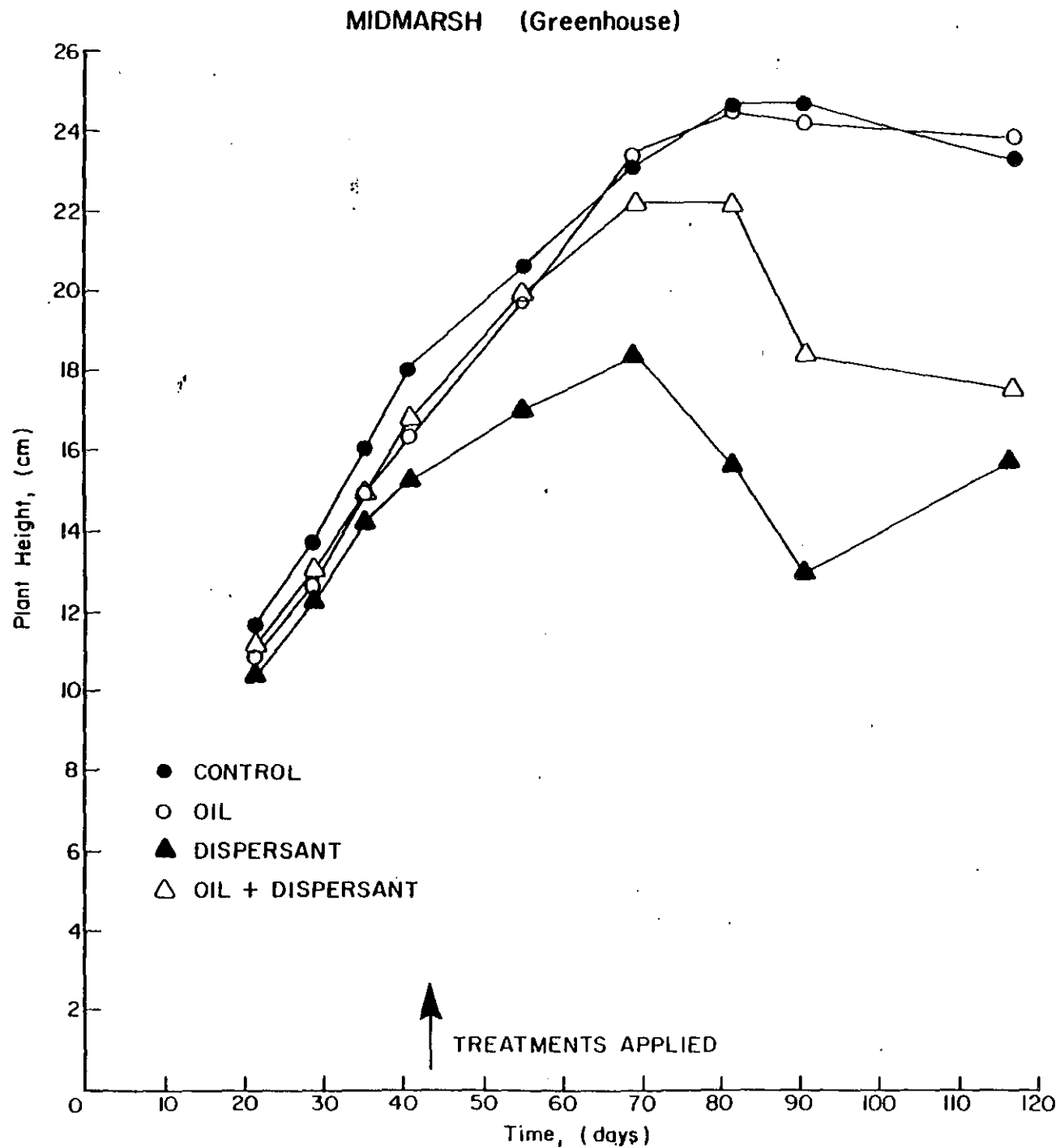


Figure 22: Mean *Spartina alterniflora* shoot height for the midmarsh zone versus time. Data are from the greenhouse study.

appeared to have little impact on the algal community. The oil and/or dispersant treatments also had little long-term impact on the other microbial zones.

Heterotrophic Activity - The null hypotheses tested in this experiment were:

- H₁: Oil has no effect on heterotrophic activity in salt marsh sediments.
- H₂: Dispersant has no effect on heterotrophic activity in salt marsh sediments.
- H₃: There is no interaction (synergistic or inhibitory) between the factors oil and dispersant, affecting heterotrophic activity in salt marsh sediments.

These hypotheses were tested for sediments from the midmarsh zone. The results of the Student-Newman-Keuls multiple range tests and the two-way analysis of variance procedures used to test these hypotheses are summarized in Appendix 14.

Data on heterotrophic activity (measured by carbon dioxide evolution) are presented in Figure 37, Appendix 14). Pre-treatment heterotrophic activity rates were similar for all treatments. The oil treatment significantly reduced heterotrophic activity on days 96 and 99 (4 and 7 days after treatment applications). Heterotrophic activity returned to normal 13 days after oiling (day 105) and was significantly higher ($\alpha = 0.05$, $df=4$) than the controls 26 and 35 days after oiling (days 118 and 127). Based on these results, the null hypothesis H₁ was rejected.

The dispersant treatment had no significant impact on heterotrophic activity four days after treatment application (day 96), however, 7, 13 and 26 days after treatment (days 99, 105 and 118), heterotrophic activity was significantly higher ($\alpha = 0.05$, $df=4$) than control values. Heterotrophic activity returned to control levels within 35 days of treatment application (day 127). Based on these results the null hypothesis H₂ was rejected.

The oil plus dispersant treatment resulted in heterotrophic activity which was intermediate between the oil and dispersant treatments. Four days after treatment (day 96), heterotrophic activity was significantly reduced ($\alpha = 0.05$, $df=4$) relative to the control and was very similar to the oil treatment. Heterotrophic activity for the oil plus dispersant treatment was not significantly different from the control 7, 13 and 26 days after treatment applications (days 99, 105 and 118). Thirty-five days after treatment applications (day 127), heterotrophic activity was significantly

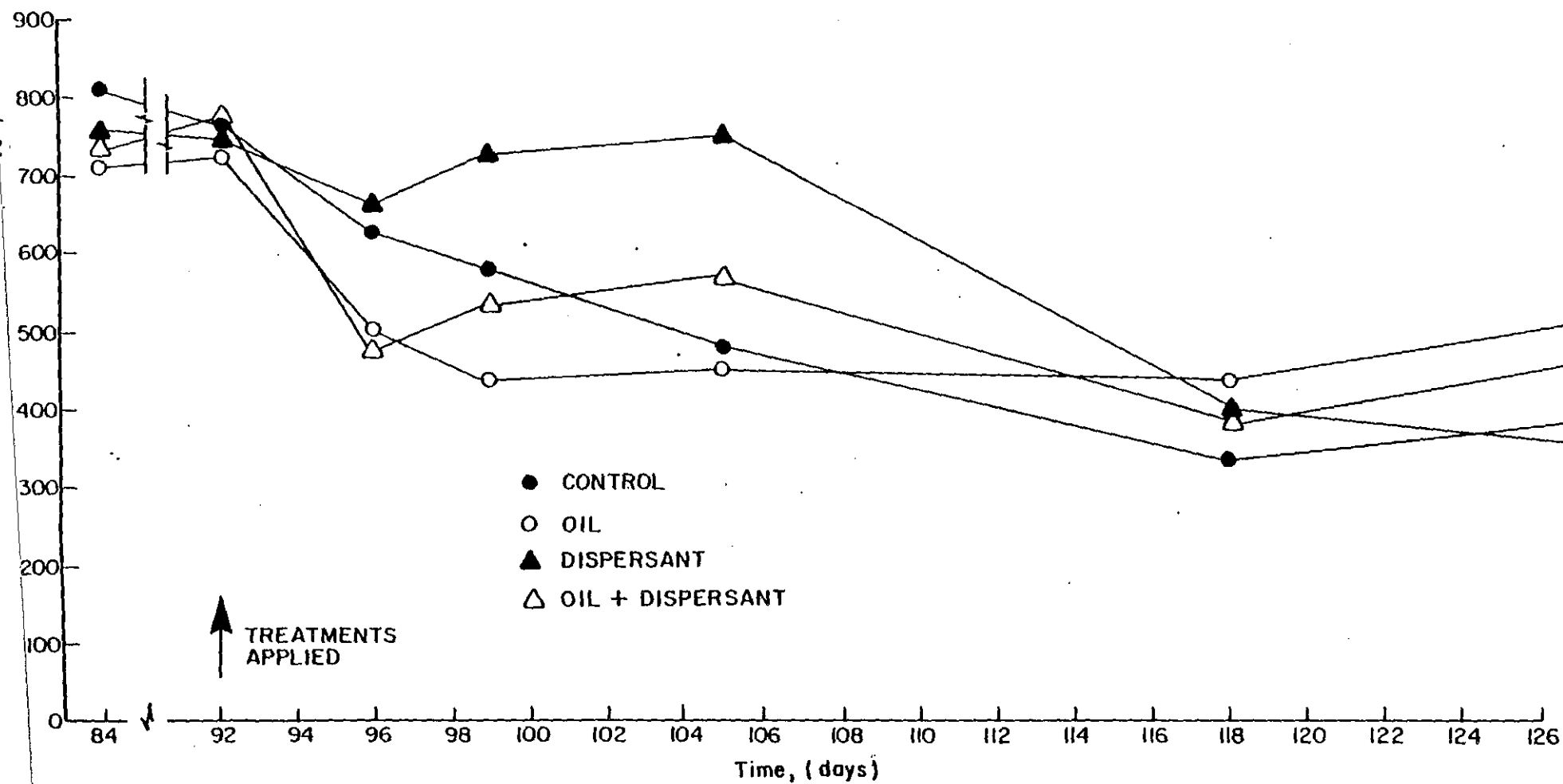


Figure 37: Plot of heterotrophic activity versus time for control, oil and/or dispersant treated midmarsh sediments. Treatments were applied on day 92.

higher ($\alpha = 0.05$, $df=4$) in the oil plus dispersant treated microcosms than in the controls. Significant interaction of oil and dispersant ($\alpha = 0.01$, $df=4$) was noted on only one sampling date (day 118). The combination of oil and dispersant produced a synergistic enhancement of toxicity. These results would suggest that the null hypothesis H_3 should be rejected, however, it should be remembered that the interaction was only temporary.

The oil and oil plus dispersant treatments evoked a two phase response from the heterotrophic bacteria. Initially, heterotrophic activity was reduced, presumably by the toxic effects of short chain hydrocarbons in the oil or by smothering of bacterial colonies. This was followed by a significant increase in heterotrophic activity relative to the controls. This increase may be associated with the weathering of short chain hydrocarbons or the colonization of the oil by bacteria capable of using it as a carbon source. A temporary reduction in heterotrophic activity was not noted for the dispersant treatments, instead, a significant flush of heterotrophic activity lasting approximately 27 days was observed. Jenkinson (1966) noted a similar increase in heterotrophic activity (carbon dioxide evolution) after the fumigation of soils, which was attributed to the death of microbial cells followed by accelerated growth of microbes growing on nutrients released by the killed cells. These effects were transient as in this case.

The results for the oil and oil plus dispersant treatments noted here are similar to those obtained by Dutka and Kwan (1984) for heterotrophic bacteria in freshwater ponds. They found that bacterial populations were reduced for approximately one month after oiling, however, within 84 days the oil treated ponds had higher bacterial populations relative to the control pond. Dispersed oil in their study had a stimulatory affect on heterotrophic activity which lasted approximately one month. This response is more similar to the dispersant treatment in this study than the oil plus dispersant treatment. This may be the result of differences in oil and dispersant application ratios. Dutka and Kwan (1984) used a 5:1 oil:dispersant ratio while a 10:1 ratio was used in this study. Dispersant effects would be expected to be more prominent in their oil plus dispersant treatment than in ours, which was the case. Increases in bacterial populations in Dutka and Kwans' (1984) study were attributed to increased carbon availability or elimination of predators.

Nitrogenase Activity and Composition of the Mud Surface Microbial Community - The null hypotheses tested in this experiment were:

H_1 : Oil has no effect on nitrogenase activity.

H_2 : Dispersant has no effect on nitrogenase activity.

H₃: There is no interaction (synergistic or inhibitory) between the factors oil and dispersant, affecting nitrogenase activity.

These hypotheses were tested in each of the three vegetation zones. The results of the Student-Newman-Keuls multiple range tests and the two-way analysis of variance procedures used to test these hypotheses are summarized in Appendix 15.

Nitrogenase activity (acetylene reduction) was highest in the midmarsh zone (Tables 19-21), ranging from 2 to 17 times higher than creek edge rates and 2 to 81 times higher than high marsh rates when control values from each zone were compared. Within treatment variation was very high for all vegetation zones. Nitrogenase activity for light and dark incubations within treatments were generally similar in all vegetation zones. Some exceptions were noted in the creek edge and high marsh zones.

Light and dark nitrogenase activity for the oil and oil plus dispersant treatments in the creek edge zone remained stable throughout the experiment (Table 19, Appendix 15). Nitrogenase activity for the dispersant treatment was significantly depressed ($\alpha = 0.05$, $df=4$) three days after treatment applications, then increased until the end of the experiment. No significant differences between treatments were noted on other sampling dates. Based on these results, the null hypothesis H₁ was retained while H₂ was rejected. The transient nature of the dispersant impact, however, suggests that in the long term dispersant toxicity is minimal. No significant interactions were noted between the oil and dispersant, therefore, the null hypothesis H₃ was retained.

In the midmarsh zone, oil and/or dispersant treatments had no significant impact on light or dark nitrogenase activity 3 days after treatment applications (Table 20, Appendix 15), however, on days 70 and 119 (27 and 76 days after treatment), nitrogenase activity for these treatments were substantially reduced in comparison to the control. This reduction was not significant on day 70 but was significant ($\alpha = 0.05$, $df=4$) for all oil and/or dispersant treatments on day 119. The null hypotheses H₁ and H₂ were therefore rejected. The fact that 1) the decline in nitrogenase activity was delayed for a number of days and 2) the degree of reduction was similar in all oil and/or dispersant treatments suggest that this decline may have been caused by secondary habitat modifications rather than direct toxicity. The rapid deterioration of vegetation in the oil and/or dispersant treated midmarsh microcosms may have modified, light regimes, nutrient status, or soil aeration, thereby affecting rates of nitrogen fixation. No significant interactions between the oil and dispersant were noted, therefore, the null hypothesis H₃ was retained.

Table 19 - Acetylene reduction rates in the midmarsh microcosms on four sampling dates. Light and dark incubation data are presented. Treatments were applied on day 43.

Treatment		Average Acetylene Reduction Activity (nanomoles C_2H_4 produced/cm ³ /hr (1 standard deviation))			
		June 4	June 23	July 17	Sept. 4
		Day 27	Day 46	Day 70	Day 119
Control	Light	2.34(1.95)	3.72(2.48)	-	1.14(1.09)
	Dark	2.39(1.96)	4.39(3.03)	4.06(3.90)	1.85(2.01)
Oil	Light	3.24(2.45)	3.44(2.43)	-	0.11(0.01)
	Dark	3.72(3.13)	5.36(3.92)	1.50(1.53)	0.18(0.16)
Dispersant	Light	5.23(2.96)	5.00(4.12)	-	0.29(0.19)
	Dark	4.94(3.21)	4.70(3.41)	1.89(1.10)	0.40(0.41)
Oil plus Dispersant	Light	4.49(3.43)	8.32(5.74)	-	0.14(0.14)
	Dark	4.74(3.08)	8.33(6.68)	0.62(0.54)	0.21(0.31)

Table 20 - Acetylene reduction rates in the creek edge microcosms on four sampling dates. Light and dark incubation data are presented. Treatments were applied on day 43.

Treatment		Average Acetylene Reduction Activity (nanomoles C_2H_4 produced/cm ³ /hr (1 standard deviation))			
		June 4 Day 27	June 23 Day 46	July 17 Day 70	Sept. 4 Day 119
Control	Light	0.21(0.18)	1.88(3.82)	-	0.15(0.18)
	Dark	0.28(0.30)	0.51(0.55)	0.24(0.25)	0.15(0.17)
Oil	Light	0.17(0.15)	0.22(0.14)	-	0.19(0.19)
	Dark	0.27(0.40)	0.26(0.26)	0.22(0.19)	0.24(0.21)
Dispersant	Light	0.21(0.29)	0.06(0.08)	-	0.43(0.60)
	Dark	0.17(0.23)	0.01(0)	0.16(0.17)	0.35(0.50)
Oil plus Dispersant	Light	0.13(0.17)	0.65(0.98)	-	0.04(0.02)
	Dark	0.10(0.12)	0.03(0.02)	0.06(0.03)	0.04(0.01)

In general, nitrogenase activity in oil and/or dispersant treated microcosms of the high marsh zone were not significantly different from the controls (Table 21, Appendix 15). There were no significant differences between treatments on any sampling date for the light incubations. For the dark incubations, nitrogenase activity was significantly elevated ($\alpha = 0.05$, $df=4$) in the oiled microcosms three days after treatment application (day 46). Although the mean nitrogenase activity rate for the oil plus dispersant treated microcosms on day 119 was much lower than the control, high variance of the data prevented it from being significantly different. Based on these results, the null hypotheses H_1 and H_2 were retained. No significant interactions were noted between the oil and dispersant, therefore, the null hypothesis H_3 was also retained.

Nitrogenase activity did not appear to be directly affected by the oil and/or dispersant treatments in any of the vegetation zones. Although significant reductions in nitrogenase activity were associated with all oil and/or dispersant treatments in the midmarsh zone, the delay in the reduction suggests that secondary environmental changes not direct toxicity were responsible. Similarly, Thomson and Webb (1984) found no reduction in nitrogenase activity following chronic exposure of salt marsh mud to oil.

Light and dark activities were of approximately equal magnitude. Heterocystous blue-green algae were absent in the creek edge and high marsh zones and were uncommon in the midmarsh zone (Tables 22-24). Non-heterocystous blue-green algae were very common in all vegetation zones as were sulfur bacteria in the midmarsh zone. These observations are similar to those of Patriquin and McClung (1978) for another Petpeswick salt marsh, and suggest that nonheterocystous blue-green algae and photosynthetic bacteria are the primary agents of nitrogen fixation.

All three vegetation zones were dominated by the same microbial types (Tables 22-24). The main differences lay in the abundances of the various types. Algae, blue-green algae photosynthetic bacteria and sulfur-oxidizing bacteria were much more common in the midmarsh zone than in either the creek edge or high-marsh zones. The large microbial population of the midmarsh zone corresponds to the high rates of nitrogen fixation noted in this zone. The abundance of the various microbial types was little affected by the oil and/or dispersant treatments. No changes were evident in the creek edge microcosms (Table 22). In the midmarsh microcosms there was a reduction in the abundance of a thick grey nonheterocystous blue-green algae in all oil and/or dispersant treatments and an increase in the abundance of sulfur bacteria associated with all oil and/or dispersant treatments (Table 23). In the high marsh microcosms

Table 21 - Acetylene reduction rates in the high marsh microcosms on four sampling dates. Light and dark incubation data are presented. Treatments were applied on day 43.

Treatment		Average Acetylene Reduction Activity (nanomoles C_2H_4 produced/cm ³ /hr (1 standard deviation))			
		June 4	June 23	July 17	Sept. 4
		Day 27	Day 46	Day 70	Day 119
Control	Light	0.05(0.04)	0.09(0.13)	-	0.28(0.27)
	Dark	0.06(0.07)	0.05(0.10)	0.07(0.06)	0.97(1.25)
Oil	Light	0.11(0.06)	0.27(0.20)	-	0.81(1.27)
	Dark	0.12(0.10)	0.25(0.20)	0.18(0.19)	1.29(2.07)
Dispersant	Light	0.07(0.04)	0.08(0.06)	-	0.31(0.23)
	Dark	0.19(0.26)	0.03(0.02)	0.08(0.14)	0.64(0.81)
Oil plus Dispersant	Light	0.10(0.09)	0.05(0.05)	-	0.09(0.06)
	Dark	0.23(0.22)	0.05(0.03)	0.09(0.15)	0.10(0.12)

Table 22 - Abundance of various algal, blue-green algal, and bacterial types in control, oil and/or dispersant treated creek edge microcosms on four sampling dates. Treatments were applied on day 43. 0=absent, 1=rare, 2=uncommon, 3=common, 4=abundant.

Morphological Classifications	Abundance															
	Control				Oil				Dispersant				Oil+Dispersant			
	Day				Day				Day				Day			
	31	57	74	119	31	57	74	119	31	57	74	119	31	57	74	119
ALGAE																
Thin Unbranched Filamentous																
Green Algae	3	3	3	3	4	3	3	4	3	4	2	4	2	2	3	3
Large Unbranched Filamen-																
tous Green Algae	0	0	2	2	0	1	0	0	0	2	0	0	0	0	0	0
Large Branched Filamentous																
Green Algae sp #1	1	0	0	0	0	0	0	0	1	0	2	3	0	0	0	0
Large Branched Filamentous																
Green Algae sp #2	0	0	0	0	0	0	0	0	0	0	0	2	0	0	0	0
Large Aseptate Branched																
Filamentous Green Algae	0	0	0	0	0	0	2	0	0	0	0	0	0	0	0	0
Unbranched Filamentous																
Brown Algae sp#1	0	0	0	1	2	0	0	0	2	0	0	0	2	0	0	0
Unbranched Filamentous																
Brown Algae sp#2	0	1	0	0	0	0	0	2	1	0	2	0	0	0	0	0
Spherical Colonies of																
reen Algae	0	4	1	0	0	0	0	0	0	1	0	0	0	1	0	0
Spherical Unicellular																
Green Algae	2	2	1	3	2	1	2	2	2	1	1	2	1	1	2	2
Diatoms	4	3	3	3	3	3	3	4	4	3	3	4	4	2	3	4
BLUE-GREEN ALGAE																
Thick Nonheterocystous																
Blue-green Algae	2	2	2	2	1	2	2	1	2	2	2	3	2	2	2	3
Thin Nonheterocystous																
Blue-green Algae	0	0	2	2	2	0	1	3	0	0	2	3	2	0	0	2
Thick Greyish Non-																
heterocystous Blue-green																
Algae	0	1	0	2	0	0	0	0	0	0	2	2	0	0	0	0
Spiral Nonheterocystous																
Blue-green Algae	0	1	2	0	0	4	3	0	0	0	0	1	0	0	0	1
Coccoid Nonheterocystous																
Blue-green Algae	0	0	3	0	0	0	1	0	0	0	0	0	0	0	0	0
BACTERIA																
Pink Photosynthetic																
Bacteria	0	0	0	2	0	0	0	3	0	0	0	2	0	0	0	2

Table 23 - Abundance of various algal, blue-green algal, and bacterial types in control, oil and/or dispersant treated midmarsh microcosms on four sampling dates. Treatments were applied on day 43. 0=absent, 1=rare, 2=uncommon, 3=common, 4=abundant.

Morphological Classifications	Abundance															
	Control				Oil				Dispersant				Oil+Dispersant			
	Day	Day	Day	Day	Day	Day	Day	Day	Day	Day	Day	Day	Day	Day	Day	Day
	31	57	74	119	31	57	74	119	31	57	74	119	31	57	74	119
ALGAE																
Thin Unbranched Filamentous																
Green Algae sp #1	4	3	4	3	4	4	2	0	4	3	3	3	3	4	0	0
Thin Unbranched Filamentous																
Green Algae sp #2	0	0	2	0	0	0	1	0	0	0	0	0	0	0	0	0
Thick Unbranched Filamentous																
Green Algae	0	0	0	0	0	0	0	0	0	0	0	0	0	2	0	0
Large Branched Filamentous																
Green Algae	0	2	3	0	0	0	2	0	0	0	0	0	3	0	0	0
Unbranched Filamentous																
Brown Algae sp #1	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0
Unbranched Filamentous																
Brown Algae sp#2	0	0	1	0	0	0	0	0	0	0	0	1	1	0	0	0
Branched Filamentous																
Brown Algae	0	0	0	0	0	0	0	0	0	0	0	0	0	2	0	0
Spherical Colonies of																
Green Algae	4	4	2	2	3	2	1	1	3	2	1	3	1	2	2	0
Thallus-like Colonies of																
Green Algae	0	2	2	0	2	0	0	0	0	0	0	0	0	0	0	0
Diatoms	4	3	3	3	4	1	3	4	4	2	2	4	4	1	3	4
BLUE-GREEN ALGAE																
Thick Nonheterocystous																
Blue-green Algae	3	3	2	0	3	1	2	3	3	4	4	4	3	3	3	3
Thick Greyish Non-																
heterocystous Blue-green																
Algae	0	3	4	4	0	2	3	2	0	1	1	3	0	0	0	3
Thin Nonheterocystous																
Blue-green Algae	0	1	0	4	0	1	0	2	2	1	0	0	2	1	0	4
Spiral Nonheterocystous																
Blue-green Algae	2	3	3	2	1	3	4	4	2	0	1	4	0	0	2	3
Thin Heterocystous																
Blue-green Algae	0	0	2	2	1	0	2	0	1	1	0	0	0	0	0	0
BACTERIA																
Sulfur Bacteria	0	2	0	0	1	3	4	3	3	3	4	3	4	2	3	3
Pink Photosynthetic																
Bacteria	0	0	0	4	0	0	0	3	0	1	4	4	0	0	0	3

Table 24 - Abundance of various algal, blue-green algal, and bacterial types in control, oil and/or dispersant treated high marsh microcosms on four sampling dates. Treatments were applied on day 43. 0=absent, 1=rare, 2=uncommon, 3=common, 4=abundant.

Morphological Classifications	----- Abundance -----															
	Control				Oil				Dispersant				Oil+Dispersant			
	Day				Day				Day				Day			
	31	57	74	119	31	57	74	119	31	57	74	119	31	57	74	119
ALGAE																
Unbranched Filamentous																
Green Algae	1	0	3	0	0	0	1	2	0	0	0	3	0	2	0	0
Large Branched Filamentous																
Green Algae sp #1	0	0	3	3	2	0	0	1	1	0	2	0	2	0	0	0
Large Branched Filamentous																
Green Algae sp #2	0	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0
Large Aseptate Branched																
Filamentous Green Algae	0	3	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Unbranched Filamentous																
Brown Algae sp #1	3	0	0	0	3	1	0	0	3	0	0	0	2	0	0	0
Unbranched Filamentous																
Brown Algae sp#2	0	0	1	0	0	0	0	0	1	0	0	0	0	0	0	0
Branched Filamentous																
Brown Algae	0	0	0	0	0	0	1	2	0	0	1	0	0	0	1	0
Spherical Colonies of																
Green Algae	0	1	0	0	2	2	1	0	1	3	0	0	0	1	0	0
Spherical Unicellular																
Green Algae	3	0	0	2	2	0	2	3	3	0	2	2	2	0	3	2
Diatoms	4	3	3	4	3	3	3	4	4	3	3	4	4	2	3	4
BLUE-GREEN ALGAE																
Thick Nonheterocystous																
Blue-green Algae	2	1	2	2	3	1	3	2	2	2	0	1	3	0	2	3
Thick Greyish Non-																
heterocystous Blue-green																
Algae	0	0	3	4	0	0	2	1	0	0	2	1	0	0	1	0
Thin Nonheterocystous																
Blue-green Algae	2	3	3	4	1	0	3	3	0	0	2	2	3	0	0	2
Spiral Nonheterocystous																
Blue-green Algae	12	2	1	2	0	0	2	0	0	0	0	0	0	0	0	0
BACTERIA																
Pink Photosynthetic																
Bacteria	0	0	0	2	0	0	0	2	0	0	0	2	0	0	0	2

there were reductions in the abundances of a thin nonheterocystous blue-green algae and a spiral shaped non- heterocystous blue-green algae associated with all oil and/or dispersant treatments (Table 24).

Comparison of Microbial Effects Noted in the Field and Greenhouse Experiments

Algal and bacterial cover was the only microbial parameter studied in both the field and greenhouse experiments. There was relatively poor correspondence between the field and greenhouse results. This was probably caused by a number of factors:

- 1) Sampling intensity was insufficient to compensate for the extreme patchiness of these communities.
- 2) Sampling frequency was insufficient to intercept the short term population fluctuations of which these communities are capable.
- 3) Estimation of algal and bacterial cover in the field was extremely difficult because of the large differences in light levels below and above the macrophyte canopy; resulting in increased error of estimation.

Trends of algal and bacterial cover in the greenhouse experiment were more consistent than those of the field experiment suggesting less masking of treatment effects by sampling error. Although bottle effects associated with the microcosms may have altered microbial community structure, we consider the results from the greenhouse study to be more representative of oil and/or dispersant impacts than the field study.

CONCLUSIONS

Elevation data indicated that the field plots were located in a very narrow range of elevations and were inundated only 5 to 25% of the time. There was much overlap in the range of elevations inhabited by each vegetation zone, suggesting that the factors characterizing boundaries of the zones were related to drainage rather than submergence.

Soil aeration was not noticeably altered by the oil and/or dispersant treatments. Reducing zones (indicative of anaerobic conditions) generally increased in thickness throughout the growing season, however, this trend was also noted in control quadrats and microcosms indicating that this was a normal seasonal phenomenon. Root coloration and the volume of aerated microzone around fine roots were not altered by any of the treatments.

Soil temperature was not significantly altered by the oil and/or dispersant treatments. Soil temperature differences between vegetation zones were much larger than temperature differences between treatments within a particular vegetation zone.

Visual observations in both the field and laboratory experiments indicated that the dispersant was not effective in removing oil from the vegetation. Oil concentrations in the sediments were highly variable reflecting the spatial heterogeneity of the sediment as revealed in the soil profile descriptions. In general, oil concentrations were high in both oil and oil plus dispersant treated plots, suggesting that the dispersant had little effect on the movement of oil through the sediments. The data, however, are too variable to confirm this result definitively.

Plant height data indicated that the oil treatments had relatively little impact on average plant height in all vegetation zones. The dispersant only treatment caused the largest reductions in average plant height, closely followed by the oil plus dispersant treatment. Rapid fluctuations in average plant height appeared to be caused by higher mortality among small plants. The data suggest that the midmarsh zone is the most sensitive to all treatments while the high marsh is the least sensitive. In a similar study conducted by Baker et al. (1984), Spartina anglica height was not significantly affected by any of the oil and/or dispersant treatments. The dispersant used in their study (BP1100WD) appeared to be much less toxic than the Corexit 9527 used in this study and may account for their lack of impact.

Growth rate data from the greenhouse experiment indicated that oil was responsible for the smallest reductions in plant growth rates in all vegetation zones while the dispersant caused the largest reductions. Growth rates for oil plus dispersant treated plants were generally higher than expected. Pooling of oil around the meristematic tissues at the base of the plant may have protected these tissues from the relatively more toxic dispersant. Growth rates for oil plus dispersant treated plants appeared to recover late in the growing season, however, large increases in plant mortality associated with this treatment countered this benefit.

Plant stem density was affected most by the oil plus dispersant treatment and least by the oil treatment in the midmarsh, and creek edge zones. The relative toxicity of the dispersant treatment varied between the field and greenhouse experiments. Dispersant was less toxic in the field experiments than in the greenhouse experiments. In the high marsh, oil caused the largest reduction in stem density while the dispersant treatment caused the smallest reductions. The midmarsh zone was extremely sensitive to all oil

and/or dispersant treatments with more than 90% reductions in stem density associated with all of the treatments in both the field and greenhouse experiments. Baker et al. (1984) noted a similar trend for Spartina anglica communities in Great Britain treated with oil, dispersant and oil followed by dispersant.

In the creek edge zone, oil had no significant impact on above-ground biomass production. The dispersant and oil plus dispersant treatments were generally equal in their impact, however, the oil plus dispersant treatment was somewhat less toxic in the greenhouse experiment than in the field experiment. Delaune et al. (1984) noted a similar trend for Spartina alterniflora in Louisiana. Oil alone did not significantly reduce above ground biomass; however, when oil and dispersant were applied together (without flushing) a significant reduction in biomass was noted. In the midmarsh zone all treatments caused significant reductions in above-ground biomass. In the high marsh zone all oil and/or dispersant treatments were approximately equal in their impact and caused relatively little reduction in above ground biomass. Overall the oil and/or dispersant treatments had the most impact in the midmarsh zone and the least impact in the high marsh zone.

Spartina cover values in the creek edge and midmarsh zones was reduced least by the oil treatments and most by the oil plus dispersant treatment. In the greenhouse experiment the impacts of the dispersant and the oil plus dispersant treatments were equivalent, while in the field experiment the oil plus dispersant treatment was clearly more toxic. This appeared to be related to an inhibitory interaction between oil and dispersant toxicity which appeared only in the greenhouse experiment. In the high marsh zone all treatments were approximately equal in impact and caused little reduction in Spartina cover. The midmarsh zone was most susceptible to the treatments while the high marsh was least susceptible. Salicornia europaea was the only other species which occurred regularly in the study plots. Its abundance was not affected by the various treatments.

Microscopic examination of Spartina leaves indicated that the same symptoms developed in all the oil and/or dispersant treatments. The only noticeable symptom was the loss of chloroplasts over time. Treatments varied in the rates at which this chlorosis proceeded, with the fastest rates associated with either the dispersant or oil + dispersant treatment and the slowest rate associated with the oil treatment.

Fluorometry results indicated that variable fluorescence was a good predictor of long term response of the plants to treatment induced stress. One hundred second difference values were a more sensitive indicator of

plant stress than peak variable fluorescence. The former indicated stress within one day of treatment application while the latter indicated changes only when the plants were near death. Dispersant alone induced the most stress in all vegetation zones. The oil plus dispersant treatments generally induced less stress than the dispersant treatment. Oil was applied to the plants before the dispersant and may have acted as a barrier to entry of the dispersant into plant tissues. Oil treatments caused relatively little stress in all but the midmarsh zone. The midmarsh zone was most sensitive to the oil and/or dispersant and the high marsh zone was least sensitive.

Algal mats, heterotrophic bacteria and pink photosynthetic bacteria appeared to be temporarily reduced in abundance by treatments incorporating oil but were unaffected or stimulated by the dispersant treatments. Oil treatments eventually increased the abundance of heterotrophic bacteria, probably largely through the proliferation of oil degrading species, however, other species such as sulfur-oxidizing bacteria also seemed to benefit.

Nitrogen fixation was unaffected by the oil and/or dispersant treatments in the creek edge and high marsh zones, however, all treatments reduced nitrogen fixation in the midmarsh zone. Evidence suggests that this decline was probably caused by alterations in microbial microhabitat rather than by direct toxicity. Nitrogen fixation rates were highest in the midmarsh zone. Results of light and dark incubations and microscopic observation of the algal mat suggest that nonheterocystous blue-green algae and photosynthetic bacteria are the primary agents of nitrogen fixation.

It is obvious that dispersant applied to saltmarshes has deleterious effects on the vegetation, more so than oil alone (at least initially). Unfortunately, the results of the experiments did not show whether or not dispersants could accelerate the natural cleansing of oil from salt marshes and, without long term monitoring, whether or not dispersants retard the recovery of oiled saltmarshes.

Until further research is undertaken to assess the efficacy of dispersant use in saltmarshes (including spray application to slicks entering marshes, premixed oil and dispersant test plots and dispersant use in conjunction with low pressure water flushing), the use of dispersants to cleanse oiled saltmarshes cannot be recommended.

This study has also demonstrated that sensitivity to oil and dispersant varies greatly among plant communities. Variations in the sensitivities of species to oil have been noted by a number of authors (Baker 1971b, Cowell

1969, Cowell and Baker 1969). Specific differences in susceptibility to dispersants have been demonstrated by Baker et al (1984). This fact demonstrates the need to tailor oil spill response planning to the floristic composition of the threatened marsh; a view which is shared by Roberts and Robertson (1986). Results from this study suggest that the creek edge and high marsh vegetations are relatively tolerant of single oil spill events and can probably be left to cleanse themselves naturally. The midmarsh zone, however, was very sensitive to both oil and dispersant and alternative clean up measures such as low pressure flushing ought to be utilized in areas of marshes dominated by this community.

Another factor which merits consideration in oil spill response planning for salt marshes is the variation of plant sensitivity within a species which is attributable to differences in pre-oiling plant stress. This study demonstrated that stunted Spartina alterniflora in the water logged midmarsh zone was far more sensitive to all of the oil and/or dispersant treatments than tall S. alterniflora in the well drained creek edge zone.

APPENDIX I: PRINCIPLES OF FLUORESCENCE INDUCTION MEASUREMENTS

Light absorbed by the chlorophyll-a reaction center of photosystem II (the oxygen evolving part of the photosynthetic system) excites ground state electrons in the chlorophyll molecule. An excited electron very quickly (within ca 10.-10 milliseconds) loses some of its excitation energy, which is dissipated as heat. This less excited, lower energy state may then jump to a primary electron acceptor, or it may return to its ground state. When it does the latter, its energy is dissipated as fluorescent light reemitted from the molecule, or as heat. If the electron is passed to the primary electron acceptor, the lost electron is replaced by an electron taken from water (with concomittant release of free oxygen). The primary electron acceptor in the meantime loses the electron to an electron carrier which loses it to another carrier and so on and through photosystem I, until finally an electron is passed to NADP. The electron carrying-NADP then moves out of the internal membranes of the chloroplast where the chlorophyll and carriers are located, into the stroma where it participates in the reactions which convert carbon dioxide to sugar.

Because some of the excited electron energy is always lost as heat, and because there is an inverse relationship between the wavelength of light and its energy, the wavelength of light reemitted as fluorescence is longer than that of the light that was absorbed. The fluorescence wavelength is characteristic of the molecule; for chlorophyll-a under physiological conditions, there is a main band maximum at 685 nm. To measure fluorescence, the sample is subjected to light of lower light intensities, and light emission at 685 nm is monitored.

What is termed "variable fluorescence" can be thought of as an overflow phenomenon; when there is more light absorbed than can be immediately processed via the chemical route, part or most of the excited electron energy is dissipated as fluorescence.

When the plant is first exposed to light, the initial electron acceptor and the downstream carriers are empty handed and can readily receive electrons from chlorophyll. As they become saturated with electrons, their ability to process more electrons decreases, the chlorophyll reaction center cannot pass on its excited electrons, the excited electrons drop back to their unexcited ground state and fluorescence rises. It reaches a peak value within about 3 seconds of the light being turned on. During this period, the rest of the electron carrier system and the Calvin cycle become activated and begin to accept electrons from the upstream carriers and the primary electron acceptor. More electrons can be processed, therefore,

fluorescence begins to decline. Within about 10 seconds, a steady state in the processing system is established, and there is a relatively low, steady level of fluorescence (Figure 3).

The pattern of fluorescence change to this point is called the fluorescence induction curve. Over longer periods (minutes), there may be another, slower increase in fluorescence and a new plateau in fluorescence reached as processes further downstream from the initial events become saturated and begin to regulate the upstream flow.

Fluorescence induction is a sensitive probe for photosynthesis over very short periods, and responds to any changes affecting photosystem II activity. Thus, it has been very important in elucidation of photosynthetic mechanisms (Papageorgiou, 1975). More recently, it has been used in studies investigating the effects of various types of stress on plants. Many types of stress - water, heat, excess light, salt, chilling, freezing - result in changes in fluorescence induction characteristics long before there are changes in other measureable characteristics (Smilie & Hetherington, 1983). There appear to be no studies on stress to foliage that have indicated effects on other processes without there being effects on fluorescence induction.

Changes in fluorescence curves brought about by stress can be interpreted to provide presumptive evidence about the way in which the stress is affecting photosynthesis. For example, a reduced peak in fluorescence could indicate damage to the light receptor system; increased peak fluorescence could be due to damage on the carrier side of PS II close to the reaction centre, and a slow decline from the peak could be due to damage further downstream.

To our knowledge, this technique has not been previously used in studies of oil and/or dispersant damage to plants. We had several objectives in applying it in this study:

- i) We wished to determine if any of the treatments affected the photosynthetic system, and how such effects varied between treatments and zones.
- ii) By comparing the fluorescence induction parameters with other criteria of stress (growth rates, flowering, etc.) measured at different times, we wished to determine if this method could be used to forecast stress effects that would otherwise be apparent only after longer intervals.
- iii) Assuming that changes in the chlorophyll induction curve would be one

of the first responses to stress, we wished to use the method to indicate how quickly after applying oil and/or dispersant, the plants begin to be stressed.

APPENDIX 2: SUMMARY TABLES OF STATISTICS FOR THE FIELD PLANT HEIGHT DATA

Table 1

Matricies of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D) and oil plus dispersant (O+D) treatments on five sampling dates in the creek edge, midmarsh and high marsh zones. Significant differences (0.05 level of significance, two degrees of freedom) between means are indicated by asterisks. Data were transformed by $\ln(x + \text{median} + 1)$. Treatments were applied on day 60.

	Creek Edge	Midmarsh	High Marsh
	No significant differences between treatments.	No significant differences between treatments	C O D O+D
Day 57			C O * D * O+D
	C O D O+D	No significant differences between treatments	C O D O+D
Day 75	C O D * * O+D * * *		C O * D * * O+D *
	C O D O+D	C O D O+D	C O D O+D
Day 89	C O * D * * O+D * * *	C O * D * * O+D * *	C O * D * * O+D * *
	C O D O+D	C O D O+D	C O D O+D
Day 102	C O D * * O+D * * *	C O D * * O+D * * *	C O * D * O+D * *
	C O D O+D	C O D O+D	C O D O+D
Day 123	C O * D * * O+D * *	C O * D * * O+D *	C O * D * O+D * *

APPENDIX 2 (Continued)

Table 2

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were transformed by $\ln(x + \text{median} + 1)$. Treatments were applied on day 60.

- * = significant at the 0.05 level of significance, two degrees of freedom.
 ** = significant at the 0.01 level of significance, two degrees of freedom.

F-ratios

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Day 57	4.61*	0.29	13.0 **
Day 75	11.8 **	0.41	16.2 **
Day 89	1.53	6.16*	11.0 **
Day 102	9.08**	0.09	3.15
Day 123	0.35	11.0 **	0.095
Dispersant			
Day 57	0.08	0.04	0.76
Day 75	70.5**	0.41	70.0 **
Day 89	125 **	6.16*	59.2 **
Day 102	224 **	0.09	0.37
Day 123	47.7**	11.00**	0.01
Two-way Interaction			
Oil and Dispersant			
Day 57	0.75	1.20	0.10
Day 75	7.04**	4.42*	33.5 **
Day 89	17.9 **	0.64	19.9 **
Day 102	25.1 **	10.4 **	22.7 **
Day 123	6.22 *	1.91	79.8 **

APPENDIX 3: SUMMARY TABLES OF STATISTICS FOR THE FIELD STEM DENSITY DATA

Table 1

Matricies of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D) and oil plus dispersant (O+D) treatments on four sampling dates in the creek edge, midmarsh and high marsh zones. Significant differences (0.05 level of significance, two degrees of freedom) between means are indicated by asterisks. Data were transformed by $\ln(x + 1)$. Treatments were applied on day 60.

	Creek Edge				Midmarsh	High Marsh			
	C	O	D	O+D		C	O	D	O+D
Day 26	C				No significant differences between treatments	C			
	O					O			
	D	*	*			D	*		
	O+D			*		O+D			
Day 49	C				C				No significant differences between treatments
	O				O				
	D	*	*		D	*			
	O+D			*	O+D	*			
Day 89	C				C				
	O	*			O	*			
	D	*			D	*			
	O+D	*	*	*	O+D	*	*	*	
Day 123	C				C				
	O	*			O	*			
	D	*	*		D	*			
	O+D	*	*	*	O+D	*	*	*	

APPENDIX 3 (Continued)

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were transformed by $\ln(x + 1)$. Treatments were applied on day 60.

- * = significant at the 0.05 level of significance, two degrees of freedom.
- ** = significant at the 0.01 level of significance, two degrees of freedom.

F-ratios

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Day 26	2.78	0.01	6.82**
Day 49	0.82	1.34	2.22
Day 89	134 **	209 **	39.6 **
Day 123	46.0 **	208 **	17.4 **
Dispersant			
Day 26	18.1 **	0.34	4.51*
Day 49	14.0 **	7.23**	1.50
Day 89	81.5 **	152 **	0.25
Day 123	195 **	283 **	2.05
Two-way Interaction			
Oil and Dispersant			
Day 26	3.58	0.62	0.05
Day 49	9.07**	2.21	0.19
Day 89	13.6 **	6.71*	36.4 **
Day 123	14.2 **	54.2 **	10.5 **

APPENDIX 4: SUMMARY TABLES OF STATISTICS FOR THE FIELD BIOMASS DATA

Table 1

Matrices of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D) and oil plus dispersant (O+D) treatments on five sampling dates in the creek edge, midmarsh and high marsh zones. Significant differences (0.05 level of significance, two degrees of freedom) between means are indicated by asterisks. Data were transformed by square root.

Total Above-Ground Biomass

	Creek Edge				Midmarsh				High Marsh			
	C	O	D	O+D	C	O	D	O+D	C	O	D	O+D
Day 124	C				C				C			
	O				O *				O			
	D	*	*		D *				D *			
	O+D	*	*		O+D *	*			O+D			

Reproductive Biomass

	Creek Edge		Midmarsh		High Marsh	
Day 124	No significant		No significant		No significant	
	differences		differences		differences	
	between		between		between	
	treatments		treatments		treatments	

APPENDIX 4 (Continued)

Table 2

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were transformed by square root. Treatments were applied on day 60.

* = significant at the 0.05 level of significance, two degrees of freedom.

** = significant at the 0.01 level of significance, two degrees of freedom.

Total Above Ground Biomass
F-ratios

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Day 124	3.46	27.7 **	0.13
Dispersant			
Day 124	41.6 **	49.0 **	8.59*
Two-way Interaction			
Oil and Dispersant			
Day 124	1.03	4.97	2.72

Reproductive Biomass

Two-way analysis of variance was not conducted on the field reproductive biomass data.

APPENDIX 5: SUMMARY TABLES OF STATISTICS FOR THE FIELD SPARTINA COVER DATA

Table 1

Matrices of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D), and oil plus dispersant (O+D) treatments on three sampling dates in the creek edge, midmarsh and high marsh zones. Significant differences (0.05 level of significance, two degrees of freedom) between means are indicated by asterisks. Data were untransformed. Treatments were applied on day 60.

	Creek Edge				Midmarsh				High Marsh
Day 34	No significant differences between treatments				No significant differences between treatments				No significant differences between treatments
	C	O	D	O+D	C	O	D	O+D	
Day 75	C				C				No significant differences between treatments
	O	*			O	*			
	D	*			D	*			
	O+D	*	*	*	O+D	*			
	C	O	D	O+D	C	O	D	O+D	No significant differences between treatments
Day 102	C				C				
	O	*			O	*			
	D	*	*		D	*			
	O+D	*	*	*	O+D	*			

APPENDIX 5 (Continued)

Table 2

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were untransformed. Treatments were applied on day 60.

- * = significant at the 0.05 level of significance, two degrees of freedom.
 ** = significant at the 0.01 level of significance, two degrees of freedom.

F-ratios

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Day 34	2.47	0.56	1.72
Day 75	32.2 **	32.3 **	1.20
Day 102	28.5 **	33.2 **	1.16
Dispersant			
Day 34	0.27	0.67	0.17
Day 75	39.5**	24.3 **	0.27
Day 102	108 **	48.9 **	0.86
Two-way Interaction			
Oil and Dispersant			
Day 34	2.15	0.04	0.08
Day 75	1.78	3.25	0.36
Day 102	1.49	18.33**	4.33

APPENDIX 6: SUMMARY TABLES OF STATISTICS FOR THE GREENHOUSE
PLANT HEIGHT DATA

Table 1

Matrices of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D), and oil plus dispersant (O+D) treatments on nine sampling dates in the creek edge, midmarsh and high marsh zones. Significant differences (0.05 level of significance, four degrees of freedom) between means are indicated by asterisks. Data were untransformed. Treatments were applied on day 43.

	Creek Edge				Midmarsh	High Marsh			
	C	O	D	O+D		C	O	D	O+D
Day 22	C				No significant differences between treatments	C			
	O					O			
	D					D	*	*	
	O+D	*	*			O+D			
Day 28	C				No significant differences between treatments	C			
	O					O			
	D	*				D	*	*	
	O+D		*	*		O+D			*
Day 35	C				No significant differences between treatments	C			
	O					O			
	D	*				D	*	*	
	O+D		*	*		O+D			*
Day 41	C				C	C			
	O				O	O			
	D	*			D	D	*	*	
	O+D		*	*	O+D	O+D			*
Day 55	C				C	C			
	O	*			O	O	*		
	D	*	*		D	D			
	O+D			*	O+D	O+D			

APPENDIX 6 (Continued)

	C O D O+D	C O D O+D	C O D O+D
Day 68	C O * D * * O+D * *	C O D * * O+D *	C O * D * O+D *
	C O D O+D	C O D O+D	C O D O+D
Day 82	C O D * * O+D *	C O D * * O+D * * *	C O D * * O+D * *
	C O D O+D	C O D O+D	C O D O+D
Day 91	C O D * * O+D *	C O D * * O+D * * *	C O D O+D * *
	C O D O+D	C O D O+D	No significant differences between treatments
Day 117	C O D * * O+D * * *	C O D * * O+D * *	

APPENDIX 6 (Continued)

Table 2

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were untransformed. Treatments were applied on day 43.

* = significant at the 0.05 level of significance, four degrees of freedom.

** = significant at the 0.01 level of significance, four degrees of freedom.

F-ratios

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Day 22	3.31	0.09	1.25
Day 28	6.71**	0.14	2.49
Day 35	4.83*	0.01	6.81**
Day 41	5.15*	0.02	3.17
Day 55	1.11	0.50	5.22*
Day 68	3.79	2.43	5.53*
Day 82	9.41**	1.26	0.44
Day 91	7.40**	1.52	0.06
Day 117	14.7 **	1.18	0.43
Dispersant			
Day 22	0.79	0.76	7.04**
Day 28	0.01	1.69	5.04*
Day 35	0.00	2.03	4.17*
Day 41	0.14	2.41	5.46*
Day 55	6.23*	6.69**	1.67
Day 68	5.74**	12.3 **	7.16**
Day 82	12.1 **	39.5 **	13.9 **
Day 91	21.5 **	51.0 **	12.5 **
Day 117	54.1 **	19.1 **	2.35

APPENDIX 6 (Continued)

Two-way Interaction
Oil and Dispersant

Day 22	15.1 **	2.03	1.26
Day 28	27.0 **	3.16	2.05
Day 35	21.7 **	2.67	4.28*
Day 41	16.5 **	4.53*	3.64
Day 55	27.9 **	5.79*	1.38
Day 68	34.8 **	2.06	0.82
Day 82	35.6 **	3.58	0.00
Day 91	30.9 **	5.30*	0.53
Day 117	20.8 **	1.17	0.29

APPENDIX 7: SUMMARY TABLES OF STATISTICS FOR THE GREENHOUSE
SPARTINA GROWTH RATE DATA

Table 1

Matricies of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D), and oil plus dispersant (O+D) treatments during six sampling periods in the creek edge, midmarsh and high marsh zones. Significant differences (0.05 level of significance, four degrees of freedom) between means are indicated by asterisks. Data were untransformed. Treatments were applied on day 43.

	Creek Edge				Midmarsh				High Marsh			
	No significant differences between treatments				No significant differences between treatments				No significant differences between treatments			
Day 35-41	C	O	D	O+D	C	O	D	O+D	C	O	D	O+D
Day 41-55	C	O *	D *	O+D *	C	O *	D *	O+D	C	O *	D *	O+D *
Day 55-68	C	O *	D *	O+D *	No significant differences between treatments				C	O	D	O+D
Day 68-82	C	O *	D *	O+D *	C	O	D *	O+D	C	O	D *	O+D *
Day 82-91	C	O	D *	O+D	Insufficient data for analysis				No significant differences between treatments			

APPENDIX 7 (Continued)

	C	O	D	O+D
Day 91-	C			
	O			
117	D	*	*	
	O+D	*	*	*

Insufficient
data for
analysis

C	O	D	O+D
C			
O			
D			
O+D	*	*	*

APPENDIX 7 (Continued)

Table 2

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were untransformed. Treatments were applied on day 43.

* = significant at the 0.05 level of significance, four degrees of freedom.

** = significant at the 0.01 level of significance, four degrees of freedom.

F-ratios

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Days 35-41	2.52	0.21	1.29
Days 41-55	6.47**	9.88**	8.98**
Days 55-68	6.57**	1.32	0.29
Days 68-82	1.96	2.99	0.10
Days 82-91	0.01	-	2.51
Days 91-117	0	-	1.47
Dispersant			
Days 35-41	0.29	0.57	0.01
Days 41-55	38.3 **	0.54	74.6 **
Days 55-68	47.0 **	0.27	47.6 **
Days 68-82	23.3 **	0.07	11.1 **
Days 82-91	4.04*	-	0.59
Days 91-117	8.73**	-	6.79**
Two-way Interaction			
Oil and Dispersant			
Days 35-41	0.02	0.34	0.18
Days 41-55	24.5 **	12.7 **	40.6 **
Days 55-68	15.1 **	-	0.33
Days 68-82	4.57*	-	0.54
Days 82-91	6.80**	-	0.27
Days 91-117	2.68	-	7.00**

APPENDIX 8: SUMMARY TABLES OF STATISTICS FOR THE GREENHOUSE STEM DENSITY DATA

Table 1

Matrices of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D) and oil plus dispersant (O+D) treatments on six sampling dates in the creek edge, midmarsh and high marsh zones. Significant differences (0.05 level of significance, four degrees of freedom) between means are indicated by asterisks. Data were transformed by $\ln(x + 1)$. Treatments were applied on day 43.

	Creek Edge	Midmarsh	High Marsh																																																		
Day 22	No significant differences between treatments	No significant differences between treatments	No significant differences between treatments																																																		
Day 28	No significant differences between treatments	No significant differences between treatments	No significant differences between treatments																																																		
Day 35	No significant differences between treatments	No significant differences between treatments	No significant differences between treatments																																																		
Day 55	No significant differences between treatments	No significant differences between treatments	No significant differences between treatments																																																		
Day 82	No significant differences between treatments	<table><tr><td></td><td>C</td><td>O</td><td>D</td><td>O+D</td></tr><tr><td>C</td><td></td><td></td><td></td><td></td></tr><tr><td>O</td><td>*</td><td></td><td></td><td></td></tr><tr><td>D</td><td>*</td><td>*</td><td></td><td></td></tr><tr><td>O+D</td><td>*</td><td>*</td><td></td><td></td></tr></table>		C	O	D	O+D	C					O	*				D	*	*			O+D	*	*			<table><tr><td></td><td>C</td><td>O</td><td>D</td><td>O+D</td></tr><tr><td>C</td><td></td><td></td><td></td><td></td></tr><tr><td>O</td><td>*</td><td></td><td></td><td></td></tr><tr><td>D</td><td></td><td>*</td><td></td><td></td></tr><tr><td>O+D</td><td>*</td><td></td><td>*</td><td></td></tr></table>		C	O	D	O+D	C					O	*				D		*			O+D	*		*	
	C	O	D	O+D																																																	
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O+D	*		*																																																		
Day 106	No significant differences between treatments	<table><tr><td></td><td>C</td><td>O</td><td>D</td><td>O+D</td></tr><tr><td>C</td><td></td><td></td><td></td><td></td></tr><tr><td>O</td><td>*</td><td></td><td></td><td></td></tr><tr><td>D</td><td>*</td><td></td><td></td><td></td></tr><tr><td>O+D</td><td>*</td><td></td><td></td><td></td></tr></table>		C	O	D	O+D	C					O	*				D	*				O+D	*				No significant differences between treatments																									
	C	O	D	O+D																																																	
C																																																					
O	*																																																				
D	*																																																				
O+D	*																																																				

APPENDIX 8 (continued)

Table 2

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were transformed by $\ln(x + 1)$. Treatments were applied on day 43.

- * = significant at the 0.05 level of significance, four degrees of freedom.
 ** = significant at the 0.01 level of significance, four degrees of freedom.

F-ratios

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Day 22	0.03	0.07	0.00
Day 28	0.02	0.22	0.03
Day 35	0.26	0.21	0.03
Day 55	0.74	1.30	3.29
Day 82	2.09	197 **	17.0 **
Day 106	0.02	130 **	8.62**
Dispersant			
Day 22	0.95	0.30	0.11
Day 28	1.58	0.00	0.00
Day 35	1.32	0.00	0.00
Day 55	0.46	2.05	0.27
Day 82	0.85	688 **	0.15
Day 106	6.46*	172 **	0.09
Two-way Interaction			
Oil and Dispersant			
Day 22	0.15	0.01	0.22
Day 28	0.23	0.21	0.25
Day 35	0.23	0.29	0.76
Day 55	0.00	4.55*	2.27
Day 82	0.51	197 **	0.11
Day 106	2.00	111 **	0.28

APPENDIX 9: SUMMARY TABLES OF STATISTICS FOR THE GREENHOUSE BIOMASS DATA

Table 1

Matricies of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D), and oil plus dispersant (O+D) treatments in the creek edge, midmarsh and high marsh zones. Significant differences (0.05 level of significance, four degrees of freedom) between means are indicated by asterisks. Data were transformed by square root.

		Creek Edge				Midmarsh				High Marsh			
		C	O	D	O+D	C	O	D	O+D	C	O	D	O+D
Day 118	C					C				C			
	O					O *				O *			
	D	*	*			D *	*			D *			
	O+D	*	*			O+D *	*			O+D *			

APPENDIX 9 (Continued)

Table 2

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were transformed by square root. Treatments were applied on day 43.

- * = significant at the 0.05 level of significance, four degrees of freedom.
- ** = significant at the 0.01 level of significance, four degrees of freedom.

F-ratios

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Day 118	0.28	16.2 **	4.53*
Dispersant			
Day 118	33.8 **	65.7 **	19.5 **
Two-way Interaction			
Oil and Dispersant			
Day 118	2.35	23.7 **	3.25

APPENDIX 10: SUMMARY TABLES OF STATISTICS FOR THE GREENHOUSE SPARTINA COVER DATA

Table 1

Matricies of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D), and oil plus dispersant (O+D) treatments on two sampling dates in the creek edge, midmarsh and high marsh zones. Significant differences (0.05 level of significance, four degrees of freedom) between means are indicated by asterisks. Data were untransformed. Treatments were applied on day 43.

	Creek Edge				Midmarsh				High Marsh			
	No significant differences between treatments				No significant differences between treatments				No significant differences between treatments			
Day 43	C	O	D	O+D	C	O	D	O+D	C	O	D	O+D
	C				C				C			
Day 117	O	*			O	*			O	*		
	D	*	*		D	*	*		D	*		
	O+D	*	*		O+D	*	*		O+D	*		

APPENDIX 10 (Continued)

Table 2

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were untransformed. Treatments were applied on day 60.

* = significant at the 0.05 level of significance, four degrees of freedom.

** = significant at the 0.01 level of significance, four degrees of freedom.

F-ratios

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Day 43	0.27	1.05	4.90*
Day 117	0.96	68.6 **	3.16
Dispersant			
Day 43	0.00	0.41	1.23
Day 117	50.2 **	145 **	5.23*
Two-way Interaction			
Oil and Dispersant			
Day 43	0.47	1.36	0.14
Day 117	4.84*	68.6 **	2.32

APPENDIX 11: SUMMARY TABLES OF STATISTICS FOR THE FLUOROMETRY DATA

Table 1

Matricies of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D), and oil plus dispersant (O+D) treatments on six sampling dates in the creek edge, midmarsh and high marsh zones. Significant differences (0.05 level of significance, four degrees of freedom) between means are indicated by asterisks. Data were transformed by $\ln(x + 1)$. Treatments were applied on day 43.

		Peak Variable Fluorescence			
		Creek Edge	Midmarsh	High Marsh	
Day 25	No significant differences between treatments	No significant differences between treatments	No significant differences between treatments	No significant differences between treatments	
		C O D O+D	C O D O+D		
Day 44	C	C		No significant differences between treatments	
	O *	O			
	D	D			
	O+D	O+D	*		
Day 46	No significant differences between treatments	C O D O+D		No significant differences between treatments	
		C			
		O			
		D *			
Day 69	No significant differences between treatments	C O D O+D		No significant differences between treatments	
		C			
		O *			
		D *			
Day 83	No significant differences between treatments	C O D O+D		No significant differences between treatments	
		C			
		O			
		D			
		O+D *	* *		

APPENDIX 11 Table 1 (Continued)

	Creek Edge	Midmarsh	High Marsh
	C O D O+D		
Day 95	C O D * * O+D * * *	No significant differences between treatments	No significant differences between treatments
One-Hundred Second Difference Values			
	Creek Edge	Midmarsh	High Marsh
Day 25	No Data	No significant differences between treatments	No significant differences between treatments
Day 44	No significant differences between treatments	No significant differences between treatments	C O D O+D C O D * * O+D * *
Day 46	No significant differences between treatments	C O D O+D C O D * * O+D * *	No significant differences between treatments
Day 69	C O D O+D C O D * * O+D * *	C O D O+D C O * D * O+D *	No significant differences between treatments
Day 83	C O D O+D C O D * * O+D * *	C O D O+D C O D * * O+D * *	No significant differences between treatments

APPENDIX 11 Table 1 (Continued)

	Creek Edge				Midmarsh	High Marsh
	C	O	D	O+D	No significant differences between treatments	No significant differences between treatments
Day 95	C				Oil plus dispersant data unavailable	
	O					
	D	*	*			
	O+D	*	*			

APPENDIX 11 (Continued)

Table 2

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were transformed by square root. Treatments were applied on day 43.

* = significant at the 0.05 level of significance, four degrees of freedom.

** = significant at the 0.01 level of significance, four degrees of freedom.

Peak Variable Fluorescence

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Day 25	1.66	0.75	0.01
Day 44	11.2 **	0.35	1.21
Day 46	0.33	0.05	0.41
Day 69	0.30	10.6 **	1.17
Day 83	0.56	2.27	1.14
Day 95	0.99	2.53	0.08
Dispersant			
Day 25	0.64	0.69	1.33
Day 44	0.01	9.05**	0.31
Day 46	0.33	2.12	3.45
Day 69	0.07	1.91	0.04
Day 83	1.43	14.09**	1.70
Day 95	32.7 **	1.67	0.08
Two-way Interaction			
Oil and Dispersant			
Day 25	0.82	0.49	0.56
Day 44	6.48*	0.26	0.53
Day 46	1.21	7.02*	1.16
Day 69	0.22	0.04	0.05
Day 83	0.03	2.47	0.02
Day 95	4.02	-	0.82

APPENDIX 11 (Continued)

One-Hundred Second Difference Values

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Day 25	-	1.32	0.36
Day 44	0.31	0.10	0.17
Day 46	0.92	0.19	0.02
Day 69	0.28	2.70	0.00
Day 83	0.29	0.18	0.06
Day 95	2.80	1.03	0.77
Dispersant			
Day 25	-	1.59	0.10
Day 44	3.23	8.70**	39.3 **
Day 46	6.92*	20.1 **	0.52
Day 69	16.9 **	4.12	0.46
Day 83	20.4 **	18.7 **	0.01
Day 95	29.8 **	4.12	0.19
Two-way Interaction			
Oil and Dispersant			
Day 25	-	0.59	0.26
Day 44	0.06	0.08	0.70
Day 46	0.57	1.19	0.05
Day 69	0.08	9.52**	0.03
Day 83	0.00	11.4 **	0.00
Day 95	1.05	-	0.24

APPENDIX 12: SUMMARY TABLES OF STATISTICS FOR THE FIELD ALGAL COVER DATA

Table 1

Matricies of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D), and oil plus dispersant (O+D) treatments on two sampling dates in the creek edge, midmarsh and high marsh zones. Significant differences (0.05 level of significance, two degrees of freedom) between means are indicated by asterisks. Data were transformed by $\ln(x + 1)$. Treatments were applied on day 60.

	Creek Edge				Midmarsh				High Marsh			
	No significant differences between treatments				No significant differences between treatments				No significant differences between treatments			
Day 75	C	O	D	O+D	C	O	D	O+D	C	O	D	O+D
	C				C				C			
Day 125	O				O				O			
	D	*	*		D				D			
	O+D	*	*	*	O+D		*		O+D		*	

APPENDIX 12 (Continued)

Table 2

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were transformed by $\ln(x + 1)$. Treatments were applied on day 60.

- * = significant at the 0.05 level of significance, two degrees of freedom.
 ** = significant at the 0.01 level of significance, two degrees of freedom.

Main Effects	F-ratios		
	Creek Edge	Midmarsh	High Marsh
Oil			
Day 75	1.21	0.25	2.68
Day 125	6.53**	10.9 *	3.27
Dispersant			
Day 75	2.01	2.33	0.03
Day 125	38.2 **	0.00	3.27
Two-way Interaction			
Oil and Dispersant			
Day 75	0.02	3.32	0.41
Day 125	3.82	1.76	3.27

APPENDIX 13: SUMMARY TABLES OF STATISTICS FOR THE GREENHOUSE ALGAL COVER DATA

Table 1

Matricies of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D), and oil plus dispersant (O+D) treatments on three sampling dates in the creek edge, midmarsh and high marsh zones. Significant differences (0.05 level of significance, four degrees of freedom) between means are indicated by asterisks. Data were untransformed. Treatments were applied on day 43.

	Creek Edge	Midmarsh	High Marsh
Day 39	No significant differences between treatments	No significant differences between treatments	No significant differences between treatments
Day 67	No significant differences between treatments	C O D O+D C O * D * O+D * *	No significant differences between treatments
Day 106	C O D O+D C O D * O+D	No significant differences between treatments	No significant differences between treatments

APPENDIX 13 (Continued)

Table 2

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were transformed by $\ln(x + 1)$. Treatments were applied on day 43.

- * = significant at the 0.05 level of significance, four degrees of freedom.
 ** = significant at the 0.01 level of significance, four degrees of freedom.

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Day 39	0.03	0.62	0.15
Day 67	2.82	49.9 **	0.32
Day 106	0.18	1.53	0.01
Dispersant			
Day 34	0.35	0.67	0.62
Day 67	1.54	0.00	4.15
Day 106	6.46*	1.19	0.31
Two-way Interaction			
Oil and Dispersant			
Day 39	1.43	4.59*	0.29
Day 67	0.23	1.64	0.32
Day 106	3.36	1.04	0.02

APPENDIX 14: SUMMARY TABLES OF STATISTICS FOR THE HETEROTROPHIC ACTIVITY
DATA

Table 1

Matrices of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D), and oil plus dispersant (O+D) treatments on seven sampling dates in the midmarsh zone. Significant differences (0.05 level of significance, four degrees of freedom) between means are indicated by asterisks. Data were transformed by $\ln(x + 1)$. Treatments were applied on day 92.

Midmarsh

			C	O	D	O+D
	No significant differences between treatments		C			
Day 34		Day 118	O	*		
			D	*		
			O+D			

			C	O	D	O+D
	No significant differences between treatments		C			
Day 92		Day 127	O	*		
			D		*	
			O+D	*		*

	C	O	D	O+D
	C			
Day 96	O	*		
	D		*	
	O+D	*		*

	C	O	D	O+D
	C			
Day 99	O	*		
	D		*	
	O+D			*

	C	O	D	O+D
	C			
Day 105	O			
	D	*	*	
	O+D			*

APPENDIX 14 (Continued)

Table 2

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were transformed by $\ln(x + 1)$. Treatments were applied on day 92.

- * = significant at the 0.05 level of significance, four degrees of freedom.
- ** = significant at the 0.01 level of significance, four degrees of freedom.

F-ratios

Main Effects	Midmarsh
Oil	
Day 84	1.03
Day 92	0.04
Day 96	18.9 **
Day 99	17.0 **
Day 105	5.30*
Day 118	5.61*
Day 127	25.9 **
Dispersant	
Day 84	0.25
Day 92	0.01
Day 96	0.01
Day 99	7.15*
Day 105	17.5 **
Day 118	0.07**
Day 127	3.92
Two-way Interaction	
Oil and Dispersant	
Day 84	0.47
Day 92	0.25
Day 96	0.72
Day 99	0.02
Day 105	1.96
Day 118	11.4 **
Day 127	0.21

APPENDIX 15: SUMMARY TABLES OF STATISTICS FOR THE NITROGENASE ACTIVITY DATA

Table 1

Matricies of the Student-Newman-Keuls multiple range test results for the control (C), oil (O), dispersant (D), and oil plus dispersant (O+D) treatments on four sampling dates in the creek edge, midmarsh and high marsh zones. Significant differences (0.05 level of significance, four degrees of freedom) between means are indicated by asterisks. Data were transformed by $\ln(x + 0.05)$. Treatments were applied on day 43.

Light Incubation

	Creek Edge	Midmarsh	High Marsh
Day 27	No significant differences between treatments	No significant differences between treatments	No significant differences between treatments
Day 46	No significant differences between treatments	No significant differences between treatments	No significant differences between treatments
Day 70	No data	No data	No data
Day 119	No significant differences between treatments	C O D O+D C O * D * O+D *	No significant differences between treatments

Dark Incubation

	Creek Edge	Midmarsh	High Marsh
Day 27	No significant differences between treatments	No significant differences between treatments	No significant differences between treatments

APPENDIX 15 (Continued)

	No significant differences between treatments	No significant differences between treatments	C O D O+D
Day 46			C
			O *
			D
			O+D
	No significant differences between treatments	No significant differences between treatments	No significant differences between treatments
Day 70			
	No significant differences between treatments	C O D O+D	No significant differences between treatments
Day 119		C	
		O *	
		D *	
		O+D *	

APPENDIX 15 (Continued)

Table 2

Two-way analysis of variance results. F-ratios refer to the variance accounted for by the oil and dispersant and the interaction between these factors. Data were transformed by $\ln(x + 0.05)$. Treatments were applied on day 43.

* = significant at the 0.05 level of significance, four degrees of freedom.

** = significant at the 0.01 level of significance, four degrees of freedom.

Light Incubation

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Day 27	0.22	0.00	3.33
Day 46	0.06	0.42	0.85
Day 70	-	-	-
Day 119	0.63	8.73**	0.10
Dispersant			
Day 27	0.15	2.21	0.01
Day 46	0.80	1.75	0.82
Day 70	-	-	-
Day 119	0.32	2.48	2.06
Two-way Interaction			
Oil and Dispersant			
Day 27	0.00	0.51	0.63
Day 46	1.14	0.50	1.40
Day 70	-	-	-
Day 119	1.83	2.92	2.84

Dark Incubation

Main Effects	Creek Edge	Midmarsh	High Marsh
Oil			
Day 27	0.16	0.14	1.05
Day 46	0.05	1.18	5.45*
Day 70	0.20	3.54	0.90
Day 119	0.32	6.54*	1.49

APPENDIX 15 (Continued)

Dispersant

Day 27	0.88	1.79	2.43
Day 46	7.05*	0.23	1.78
Day 70	1.53	0.97	1.30
Day 119	0.72	2.66	5.44*

Two-way Interaction

Oil and Dispersant

Day 27	0.00	0.28	0.06
Day 46	0.30	0.27	2.37
Day 70	0.49	0.11	0.17
Day 119	2.42	2.62	1.80

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ECOLOGICAL RISK ANALYSIS
IN REGARD TO OFFSHORE OIL
DEVELOPMENT AT HIBERNIA

TWO EXAMPLES:

- A) RISK ANALYSIS OF MURRE POPULATIONS
- B) DIRECT AND INDIRECT EFFECTS AT THE
ECOSYSTEM LEVELS



P. LANE AND ASSOCIATES LIMITED
Environmental Consultants



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Preface

On May 15, 1985, Mobil Oil Canada Ltd. and its partners submitted an Environmental Impact Statement (E.I.S) on the proposed development of the Hibernia Oil Field on the Grand Banks off the coast of Newfoundland. In August, they updated their E.I.S and announced that the preferred development option is one involving a gravity based structure (GBS). In June 1985, P. Lane and Associates, Limited was asked by Environment Canada to comment on the quality of the E.I.S and to identify problems arising from both catastrophic oil spills and chronic oil pollution in the marine environment. In July 1985, Environment Canada requested that two illustrative examples of population and community level analyses be conducted. This report describes the results of this study. We stress that the examples are illustrative and not comprehensive. In their limited way, however, they do demonstrate that improved methodologies can and should be developed for assessing impacts of proposed developments.

I. INTRODUCTION

Life without risk is not possible. The public frequently believe in or demand zero risk; however, in reality it is difficult to achieve even a minimal level of risk. High costs are often involved in reducing risk to so called "acceptable" levels. As technology has developed and expanded, these levels have risen. We will never live in a risk free environment. With the risk of technology also come benefits. Trade offs of risks and benefits are made daily in our society and there are numerous and often powerful special interest groups that would like to underestimate or overestimate these risks. In addition, often those who are forced to take risks are not the same individuals who benefit from the initial activity or development. In these cases, society has some difficult economic and often ethical decisions to make. Central to these decisions, and subsequent risk management, is the critical need to quantify risk objectively and to be able to estimate the risk-benefit ratios. Numerous studies have demonstrated that even our perception of acceptable risk is not very trustworthy without rigorous methodologies to quantify our intuitive notions.

Two major problems were identified with the E.I.S. for Hibernia; both relate to the inadequate impact assessment methodology that was employed. First, the concept of risk was largely ignored, although it is crucial to making wise management decisions. Formally defined, risk is the probability that something undesirable will happen. The notion of probability can be difficult to understand. It refers to the inherent uncertainties in the complex systems that surround us; especially when we want to predict future behavior of such systems. Uncertainty is a commonplace idea when discussing the weather or the stock market. Likewise, it has been used extensively in health sciences to estimate mortality, disease incidence, birth defects, mutations, etc. The nuclear regulatory and other industries have used risk analysis for a variety of safety determinations among other uses. Risk measures have also become part of legislative and management decisions related to these fields. More recently, risk is becoming an important concept in understanding how humans affect ecological systems.

Two types of risk are important for this project: 1) the probability of an oil spill, relates to engineering risk, whereas 2) given the probability that an oil spill occurs, ecological risks can be evaluated. For example, what is the probability that an important ecological component such as a seabird population will suffer a large population loss or that the structure of the pelagic ecosystem will change to a less desirable state (altered food web, lowered productivity, lowered diversity, lower stability, etc.)?

As compared to an industrial risk analysis that often emphasizes human safety, for an ecological risk analysis of an ecosystem like the Grand Banks, there are large numbers of non-human organisms that will become involuntary risk takers and that clearly will not stand to benefit from an oil-enhanced economy. Environment Canada is at least partially charged with protecting these organisms and their environments from undue risks. The challenge is to clarify the ecological risk portion of the risk-benefit ratios so that evaluation of ecological risks can be made objectively and subsequently defended in public forums. The Hibernia Development is an excellent example of the potential for conflicting perceptions of risk by

different interest groups and the best way to resolve these conflicts is to have relative levels of risk quantified as rigorously as possible. As Rowe (1983) stated, "any determination of risk must be based upon a strong foundation. This requires an orderly approach with proper consideration of all factors."

The second major problem with the E.I.S. for Hibernia is that for all impacts, the proponents use only the most direct types of effects. It is becoming readily apparent in the ecological literature, however, that not only are indirect effects important, but that they may often dominate direct ones in nature. The proponents fail to give any indication that indirect effects are important and how they might analyze and anticipate them.

The biophysical assessment in the Hibernia E.I.S. has been criticized by many as being descriptive, overly simplified, lacking in rigor, non-quantitative, etc. The document focuses on the population level of ecological organization. For this level, the proponents state that natural variability will mask any development effects yet they fail to supply any variability estimates or to illustrate how natural variability may be altered or compounded by development. They make no quantitative prediction for any potentially impacted Hibernian population nor is the concept of risk acknowledged. The authors apparently do not recognize the inevitable uncertainty of making predictions for such a complex system and no uncertainty estimates are provided. They ignore the ecosystem level of organization almost completely. Although they have rating categories for the ecosystem level, they fail to indicate the methodology used in these ratings and it appears that a vague form of intuition may have been the only method employed. All populations existing on the Grand Banks are components within a complex ecosystem and are subject to intricate cause-effect relationships. Both catastrophic and chronic development-related impacts would not affect only a single population in isolation. Perturbations enter the food web at multiple points and interact in complex feedbacks throughout the ecosystem.

A. Purpose and Significance

The purpose of this study is twofold:

- 1) To show by example how ecological risk can be quantified for an important ecological component, in this case a seabird population, and
- 2) to provide an ecosystem-level model to illustrate, given that an oil-pollution perturbation occurs, how cause and effect could operate in the Hibernia ecosystem and what associated predictions can be made for several ecological components.

Both portions of the study are illustrative and are not meant to be comprehensive or exhaustive. They do, however, provide insights that should be considered in conducting environmental assessments and in anticipating future directions such assessments should take. Environmental impact assessments are inherently difficult to do well and are also very costly. With better planning and more objective analysis, better results can be achieved for the same costs. It is important that assessment participants cooperate in developing these state-of-the-art methodologies and in insisting on high quality, rigorous applications to real world situations

such as offshore oil development on the Grand Banks.

B. Study Organisms

We conducted an ecological risk analysis using data from populations of two species of murres (Uria lomvia, the Thick-billed Murre and Uria aalge, the Common Murre). These species were selected for study for several reasons. First, murres are one of the most abundant seabirds in the Hibernia area. The large breeding colonies of Common Murres in Newfoundland account for 97% of the eastern North American population (Brown and Nettleship, 1984a; Nettleship, 1985). Thick-billed Murres breed north of the Hibernia area in summer but migrate southward to the Grand Banks and beyond for the winter (November to March). Gaston (1980) has estimated that 3.8 million Thick-billed Murres occur off of southern and eastern Newfoundland in winter. These birds undoubtedly play an important role in the Grand Banks ecosystem. The Witless Bay Seabird Sanctuary, among others in Newfoundland, provides habitat for both species and could be impacted from marine oil spills. Murres are utilized by hunters in Newfoundland and Greenland (Evans and Waterston, 1976; Wendt and Cooch, 1984) and thus, have socio-economic value.

The second reason for selecting Murres is that they are among the most vulnerable species of seabirds to oil pollution (Brown, 1982; Brown et al., 1973; Brown and Johnson, 1980). Murres feed by diving and spend much of their life in the water where they may be exposed to oil contaminants. Immediately after leaving the colony in late summer, murres begin a swimming migration towards the winter feeding grounds. For several weeks at the start of this migration, both adults and young are flightless and therefore highly vulnerable to oil spills (Tuck, 1961; Gaston and Nettleship, 1981). An oil spill at this time could potentially devastate a population. Thus, both behavioral and life-history characteristics expose murres to any pollutants present in the ocean during important periods of their life history. Murres are indeed identified as one of the Grand Banks species most at risk in the E.I.S.

Finally, murres are one of the best studied northern seabirds (Tuck, 1961; Birkhead and Hudson, 1977; Gaston and Nettleship, 1981; Kampp, 1982). The availability of detailed data on population parameters is essential for the risk model we have used (RAMAS). In addition, experts (Drs. D.N. Nettleship and R.G.B. Brown) were available for consultation and could provide first-hand insights into the ecology of these animals.

C. Acknowledgments

Dr. Phillip Cohen served as Scientific Authority for the study and gave us much useful advice during the course of the modelling. His encouragement and assistance were very much appreciated. Likewise, Mr. Alan McIver, Mr. Wayne Barchard, Ms. Susan Hall and Mr. Rodger Albright of Environment Canada, Dartmouth, Nova Scotia served on the subworking group on Risk Assessment and assisted in guiding the project to completion.

We gratefully acknowledge the participation of Dr. L. Ginzburg of Applied Biomathematics, Inc. who created the modelling approach and related RAMAS software for the population risk analysis. He provided us with the model runs used in this report. Drs. David N. Nettleship and Richard G.B.

Brown of the Canadian Wildlife Service shared their considerable expertise in seabird ecology with us. They advised us on general biology, life-history parameters, distributions and feeding relationships, and supplied us with needed data on murre populations. Dr. Ian A. McLaren of Dalhousie University made valuable suggestions on our approach, as well as seabird ecology.

Mr. Michael Paterson prepared and interpreted much of the population risk analysis for the murre and assisted in writing the final report. Mr. John Wright provided mathematical expertise. Mr. Roger Day wrote the original loop analysis program. Dr. Anthony Blouin ran the loop analysis software and assisted in the ecosystem level modelling. Mr. Kim Seil, Ms. Pamela Mahoney and Mr. Terry Collins participated in report preparation.

II. RISK ANALYSIS OF MURRE POPULATIONS

A. Methods

1. Stochastic Leslie Matrix Model (RAMAS)

We have used a new software package called RAMAS (Risk Analysis and Management Alternatives System) to conduct a risk analysis of expected changes in population sizes in two related species of seabirds common in the Hibernia area: Uria lomvia, the Thick-billed Murre, and Uria aalge, the Common Murre.

RAMAS is a Monte Carlo simulation model based on the traditional Leslie Matrix. (See Appendix A for theoretical description.) This model uses data on age-specific fecundity, survivorship, population size and age structure to predict future changes in the numbers of organisms in different age classes. RAMAS incorporates stochastic distributions for fecundity and survivorship to calculate the uncertainty of population size predictions and the risk of a population falling below a given level during a specified time period. Different impact scenarios can be examined and their effect on a population assessed. Only changes in total population size are considered below.

The population parameters used for our model of the unimpacted scenario are presented in Table 1. When these parameters were used in the model, they were computed based upon 2 year age classes. Life table parameters were selected from various studies on murre biology and are discussed in detail in Appendix B.

2. Model Assumptions

Although murre are one of the best studied northern seabirds, it was necessary to make several assumptions concerning their biology for the RAMAS models. Throughout the modelling we attempted to be ecologically realistic and conservative in our estimates. These assumptions are discussed in detail in Appendix B. Three assumptions are particularly important. These are:

a.) We have used a single life table to examine oil development effects on both Common and Thick-billed Murres. Population studies of Thick-billed and Common Murres suggest that survivorship and productivity are very similar for both species (Appendix B). Future studies, however, may reveal parameter differences. We have used data collected from both eastern and western Atlantic populations because a complete study of survivorship and fecundity for a single population in eastern Canada is not available.

b.) We have assumed that murre populations are presently stationary. It is unclear whether or not this is realistic. Thick-billed Murres may be declining in numbers because of heavy mortality from hunting, entanglement in fish nets and oiling (Evans and Waterson, 1976; Nettleship, 1977; Gaston and Nettleship, 1981; Brown and Nettleship, 1984a, Wendt and Cooch, 1984). As a result, the RAMAS results will underestimate declines in murre populations associated with oil development.

TABLE 1. Natural population parameters for the RAMAS model.

See Appendix B for a discussion of the selection of these parameters.

sex ratio (males:females)	1:1
maximum age class	28 years
mean age specific fecundity	ages 0- 4 0.00 eggs per female ages 5-28 0.74 eggs per female [coefficient of variation 15%]
mean survivorship	
juveniles	age 0 to 1 0.50 survivorship per year age 1 to 2 0.70 survivorship per year age 2 to 3 0.80 survivorship per year [coefficient of variation 10%]
adult:	age 4 to 28 0.94 survivorship per year [coefficient of variation 5%]
density-dependence function (Ricker Model)	
a) strong density dependence	$\alpha = 1.0$ $\beta = 0.000005$
b) weak density dependence	$\alpha = 1.0$ $\beta = 0.0000014$
population size	86,000 breeding pairs (=207,000 adults plus juveniles)
number of model iterations	30

In contrast to Thick-billed Murres, populations of Common Murres appear to be increasing in Newfoundland after severe population devastation by hunting in the early 1900's (Tuck, 1961; Nettleship, 1985). If so, because we have assumed stationarity, the RAMAS results will yield overestimates of population declines and thresholds. The magnitudes of any decreases or increases in the sizes of natural populations of murres are unknown.

c.) Mortality factors, such as competition, that vary with population size are known as density-dependent regulatory mechanisms. Standard Leslie Matrix models without a density dependent function usually predict either population explosion or extinction. The return time of a population after a catastrophic impact is partially a function of the strength of density-dependent regulation. Unfortunately, the exact form of density-dependent relationships and their underlying biological mechanisms are not known for any seabird population. Thus, for the RAMAS modelling, we examined the impact of oil development under both strong and weak density dependent regulation. The real-life situation probably falls between these two extremes.

3. Results

We modelled population trends in a hypothetical colony of 86,000 breeding pairs (207,000 individuals) using RAMAS. This colony is similar in size to the colony of Thick-billed Murres at Prince Leopold Island, Northwest Territories (Gaston and Nettleship, 1981) and to the colony of Common Murres at Green Island, Witless Bay, Newfoundland (74,000 breeding pairs; Nettleship, 1985).

We examined the risk to murre populations of 1) chronic effects associated with long-term, low-level exposure to oil contaminants produced by routine operations at Hibernia, and 2) catastrophic events such as a major oil spill. Table 2 lists the population parameters used in the chronic and catastrophic scenarios. The quantification of risk is different for the catastrophic and chronic scenarios. For the former, there is usually no adequate historical data base for the probability of rare events but the identification of cause and effect is usually quite clear and easy to document. In addition, the public is often most concerned over the catastrophic event such as major oil spill or the sinking of a platform. The public is also more concerned when the risk recipient is clearly indicated than when only a statistic is given. That John Doe died of lung cancer has more of an effect than the statement, "smokers have an X% higher probability of contracting lung cancer than nonsmokers". Within the environment, the public can identify with some risk recipients (birds, fish and mammals) more easily than others (polychaetes, tunicates and decapods). In the chronic scenario, the common risk from small recurring events might be much greater than for a single catastrophic event. For example, a single plane crash might kill 100 people and receive world-wide news coverage, while 50,000 fatalities from car accidents each year may be ignored. By analogy, chronic pollution can represent a much greater risk to an environment and over the long term cause system failure. Although the data base on effects and their probabilities is better for chronic events than catastrophic ones, the problem is that cause and effect are much more difficult to delineate in the chronic situations. This is especially true in a cumulative impact situation when there are many pollutants and sources of perturbation with intricate causal links and multiplicative effects.

1. Natural Scenario

The results of all the simulations are presented in Figures 1 to 9. Predictions of abundance levels for natural (unimpacted) scenarios are contrasted with those for chronic oil pollution on Figures 1 to 3 and with the catastrophic oil-impact scenarios on Figures 6 to 9. Figures 4 and 5 present predictions of risk for natural and chronic scenarios.

To ensure initial stationarity a maximum age of 28 years was used for all scenarios. The equilibrium density of the population under strong density-dependent regulation is slightly higher than that under weak density-dependent regulation. This is because slight adjustments in juvenile survivorship (first age class) were made to the life table for the strong density-dependent cases to ensure stationarity or zero growth.

TABLE 2. Impact scenarios for the RAMAS models

I Chronic Scenarios

	weak density dependence	strong density dependence	years model allowed to run
1) decrease survivorship of all 2 year age classes by:	.02 .02 .05 .07 .10 .10	.02 .05 .07 .10 .20	20 60 20 20 20 60 20
2) decrease fecundity of all 2 year age classes by:	.10		60

II Catastrophic Scenarios

	weak density dependence	strong density dependence	years model allowed to run
1) reduce initial population size by:	5% 10%	5% 10%	60 60
(all age classes reduced similarly)	20% 90%	20% 90%	60 60
2) decrease survivorship by 0.02 and reduce initial population size by:		5% 20%	60 60

Simulation runs for RAMAS are begun with a fixed initial population whose future population size is subject to continuous uncertainty. The resultant variation in population predictions (expressed as ranges and confidence intervals) will tend to accumulate over time. This effect is most obvious in the graphs depicting trends for the unimpacted natural populations.

In both nature and the RAMAS model, the absolute magnitude of population changes through birth and mortality are proportional to population size. Therefore, in graphs depicting increases or declines of population size, the confidence intervals and ranges tend to increase or decrease proportionally with abundance levels. In figures depicting large

Figure 1. Expected changes in the total abundance (excluding age class 0) of murres over 20 years of chronic oil pollution with strong density dependent regulation.

- A. decrease of survivorship of each two year age class by .02.
- B. decrease of survivorship of each two year age class by .05.
- C. decrease of survivorship of each two year age class by .07.
- D. decrease of survivorship of each two year age class by .10.
- E. decrease of survivorship of each two year age class by .20.

Short vertical bars represent 95% confidence intervals and wide vertical bars are ranges. For clarity, wide bars are omitted for some times.

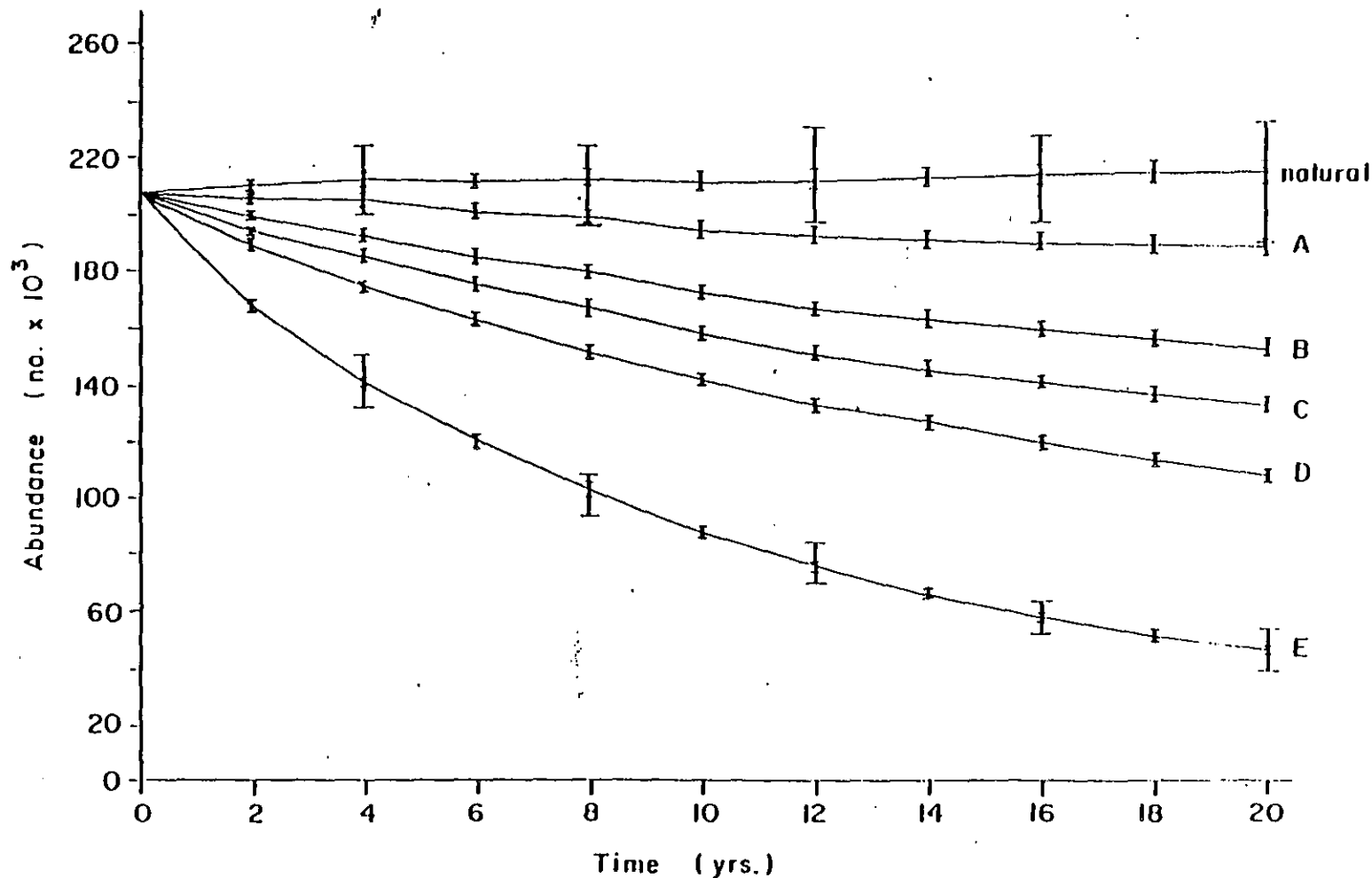


Figure 2. Expected changes in the total abundance (excluding age class 0) of murre over 20 years of chronic oil pollution with weak density dependent regulation.

A. decrease of survivorship of each two year age class by .02.

B. decrease of survivorship of each two year age class by .05.

C. decrease of survivorship of each two year age class by .07.

D. decrease of survivorship of each two year age class by .10.

Short vertical bars represent 95% confidence intervals and wide vertical bars are ranges. For clarity, wide bars are omitted for some times.

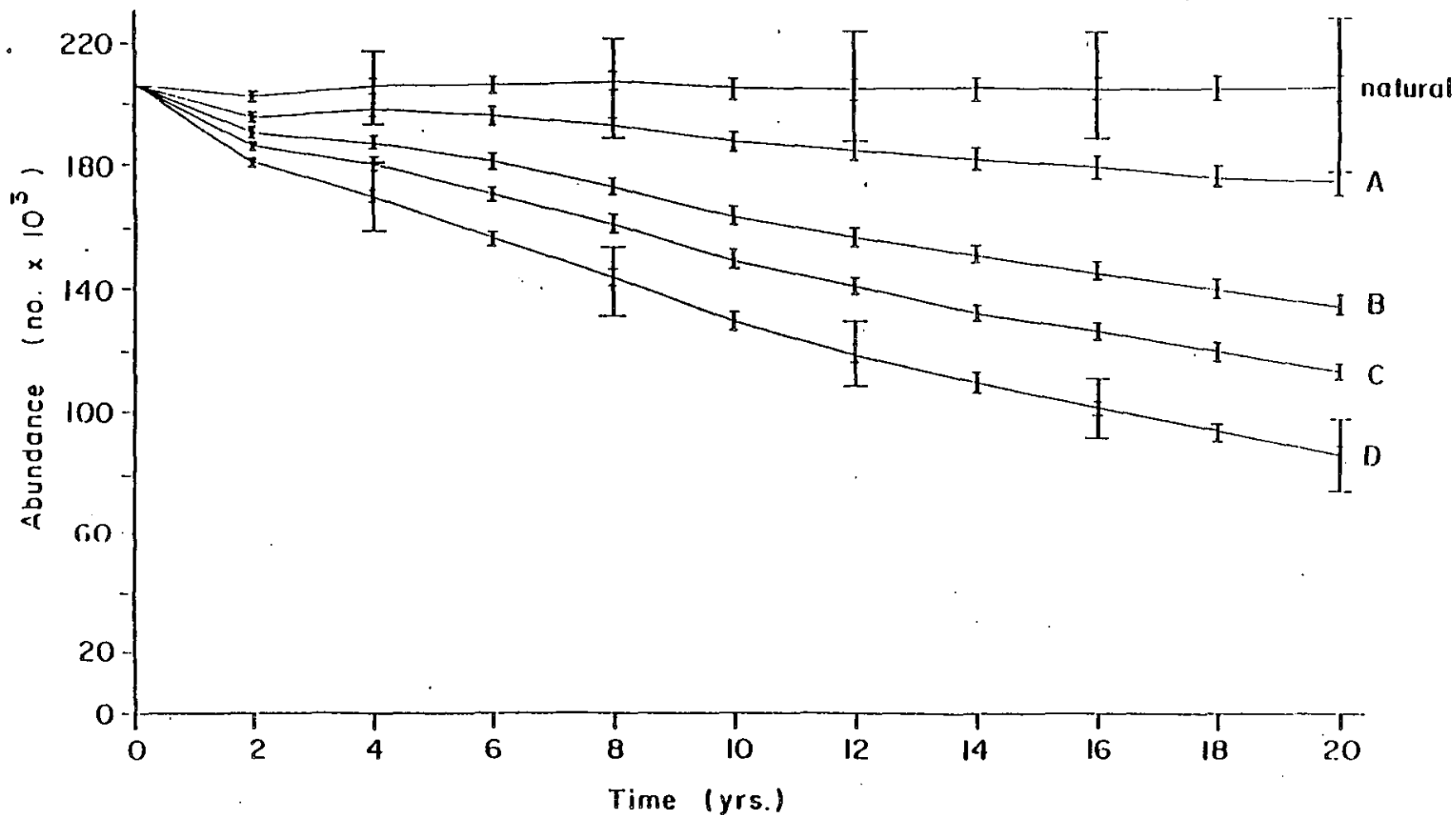


Figure 3. Expected changes in the total abundance (excluding age class 0) of murre over 60 years of chronic oil pollution with weak density dependent regulation.

A. decrease of fecundity by 0.10 for each two year age class.

B. decrease of survivorship of each two year age class by 0.02.

C. decrease of survivorship of each two year age class by 0.10.

Ranges and 95% confidence intervals are presented for representative points only. The short vertical bars are 95% confidence intervals and wide vertical bars are ranges.

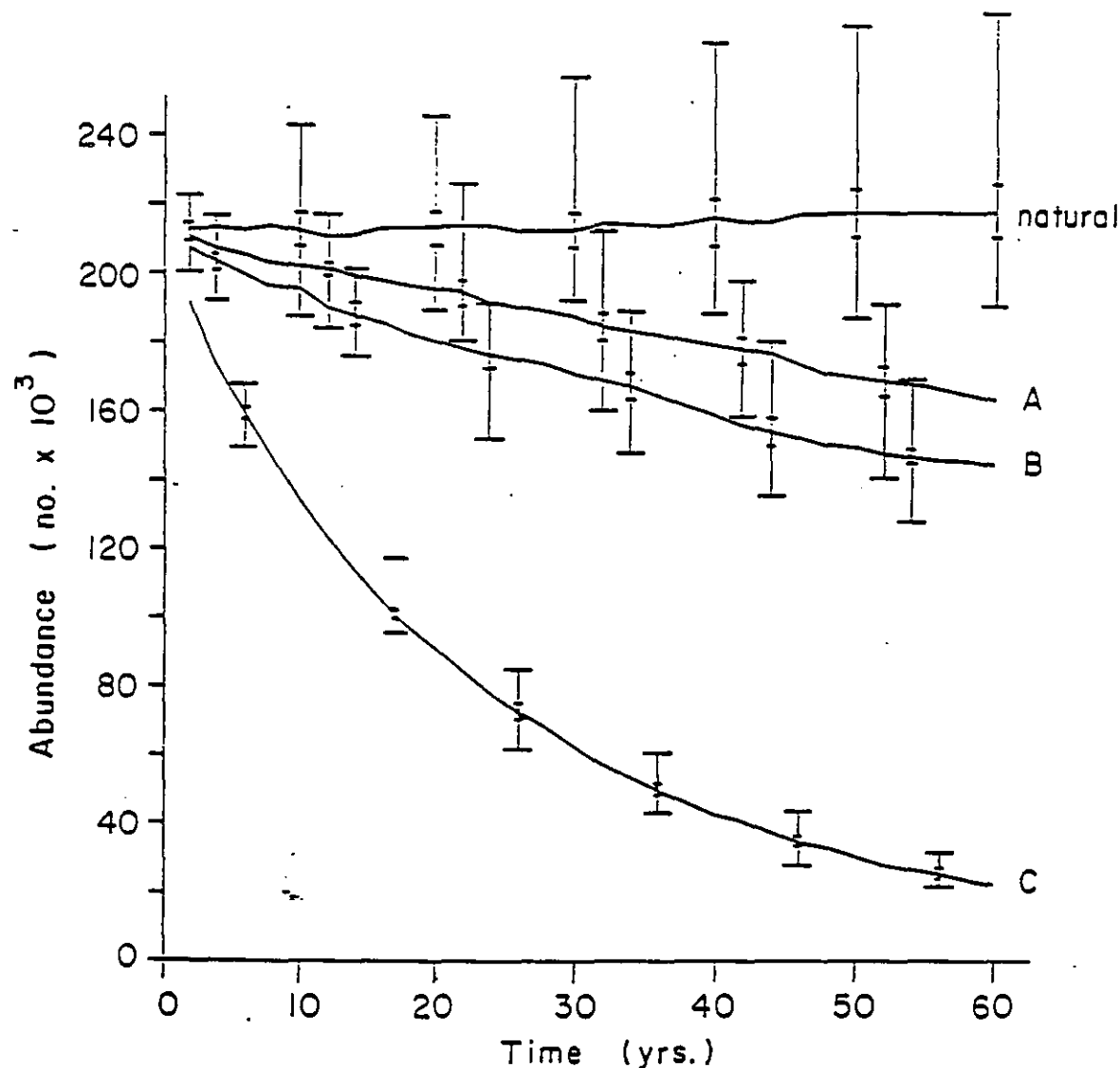


Figure 4. Probability of the population falling below a given size at least once during a 20 year period according to RAMAS predictions.

- A. natural (unimpacted) scenario
- B. decrease of survivorship every two year age class by 0.02.
- C. decrease of survivorship every two year age class by 0.05.
- D. decrease of survivorship every two year age class by 0.07.
- E. decrease of survivorship every two year age class by 0.10.
- F. decrease of survivorship every two year age class by 0.20.

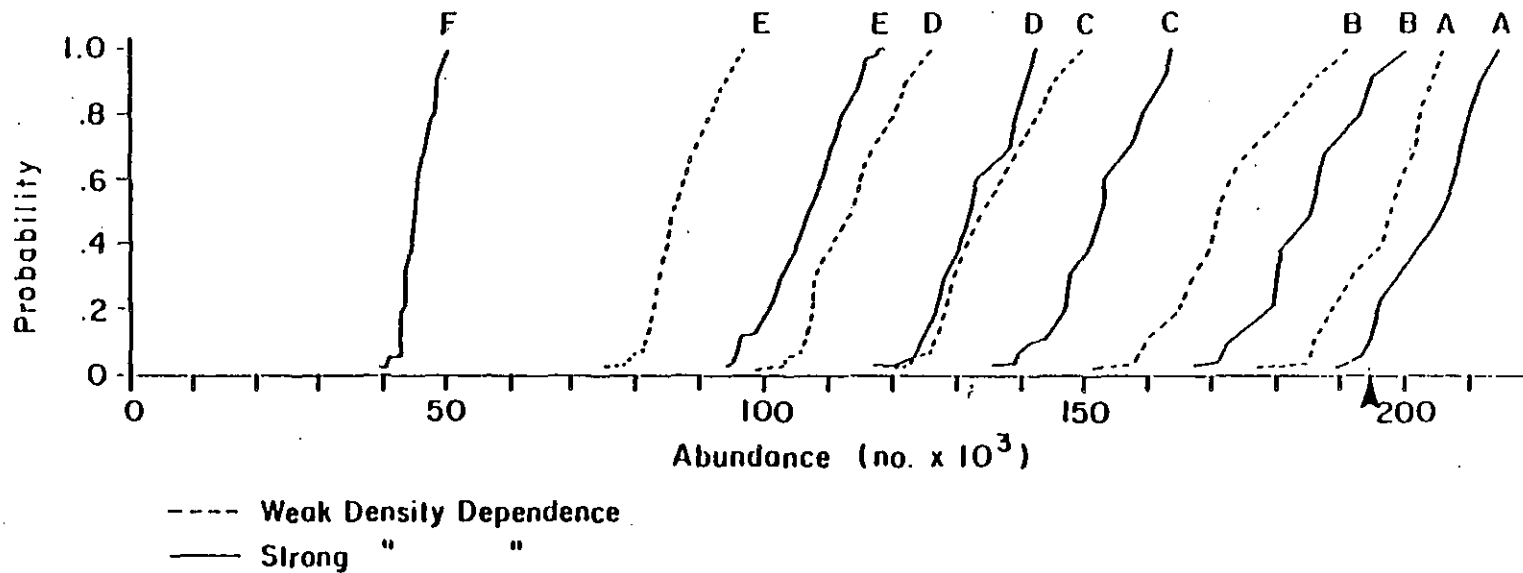


Figure 5. Probability of the population falling below a given size at least once during a 60 year period, according to RAMAS predictions.

- A. natural (unimpacted) scenario
- B. decrease of fecundity by 0.10.
- C. decrease of survivorship of every two year age class by 0.02.
- D. decrease of survivorship of every two year age class by 0.10.

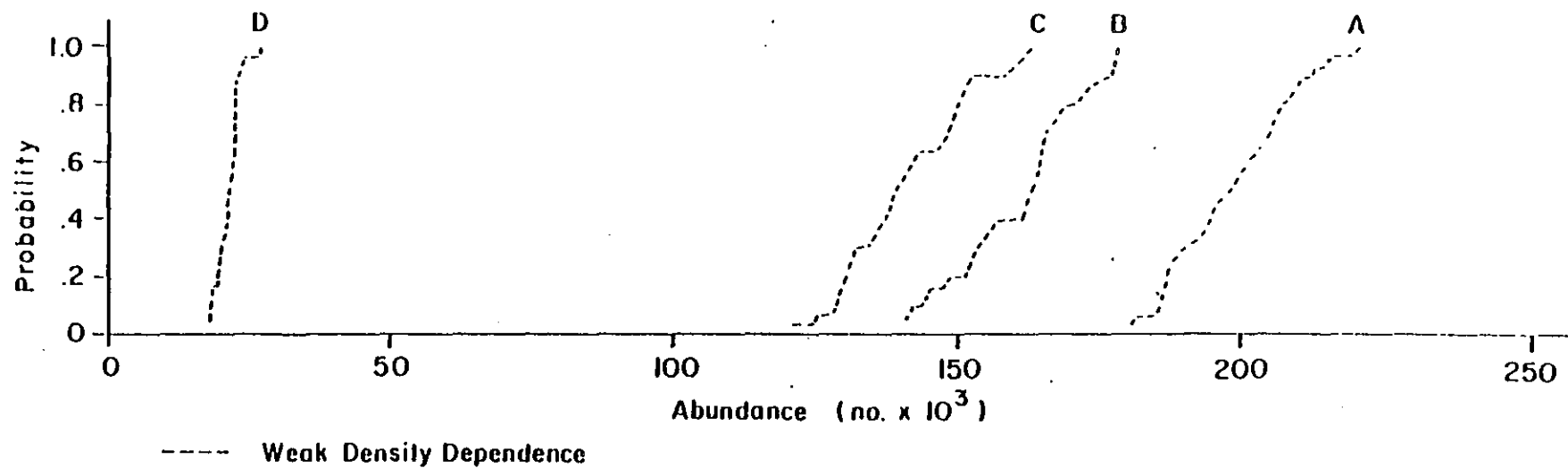


Figure 6. Expected changes in the total abundance (excluding age class 0) of murre over 60 years after a catastrophic decrease of population numbers (initial) and with strong density-dependent regulation.

- A. catastrophic decrease of initial population size by 5%
- B. catastrophic decrease of initial population size by 20%
- C. catastrophic decrease of initial population size by 5% with 0.02 decrease of survivorship of each age class
- D. catastrophic decrease of initial population size by 20% with 0.02 decrease of survivorship of each age class

The run for the natural (unimpacted scenario) was continued for only 20 years. Ranges and 95% confidence intervals are presented for representative points only. The short vertical bars are 95% confidence intervals and wide vertical bars are ranges.

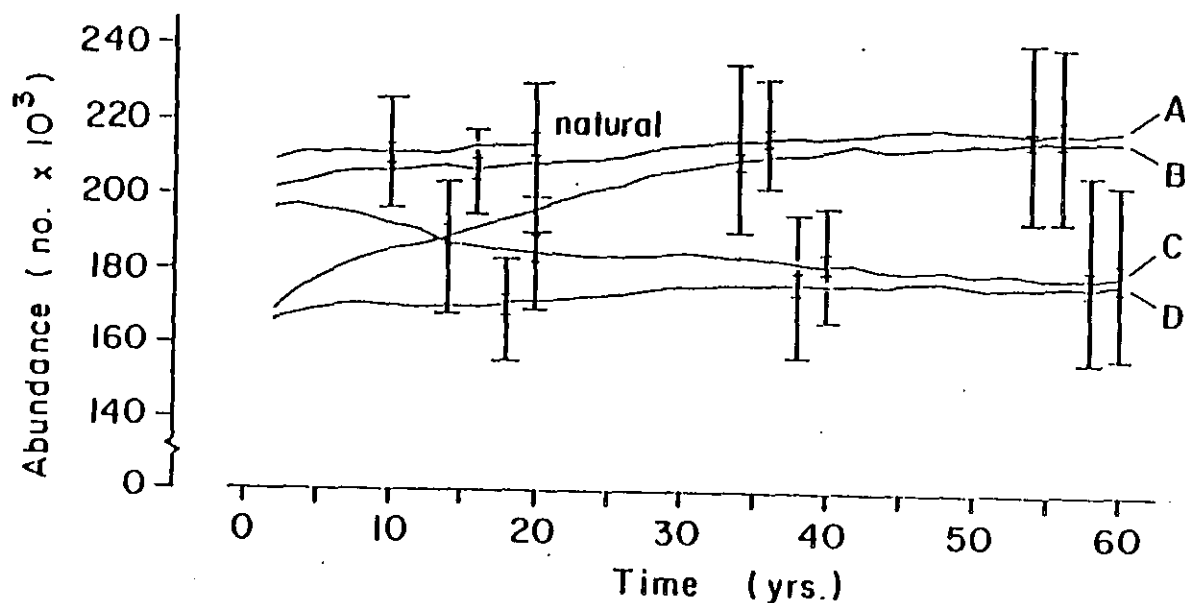


Figure 7. Expected changes in the total abundance (excluding age class 0) of waxes over 60 years after a catastrophic decrease of population numbers (initial) and with strong density dependent regulation.
A. catastrophic decrease of initial population size by 10%
B. catastrophic decrease of initial population size by 90%
Ranges and 95% confidence intervals are presented for representative points only. The short vertical bars are 95% confidence intervals and the wide vertical bars are ranges.

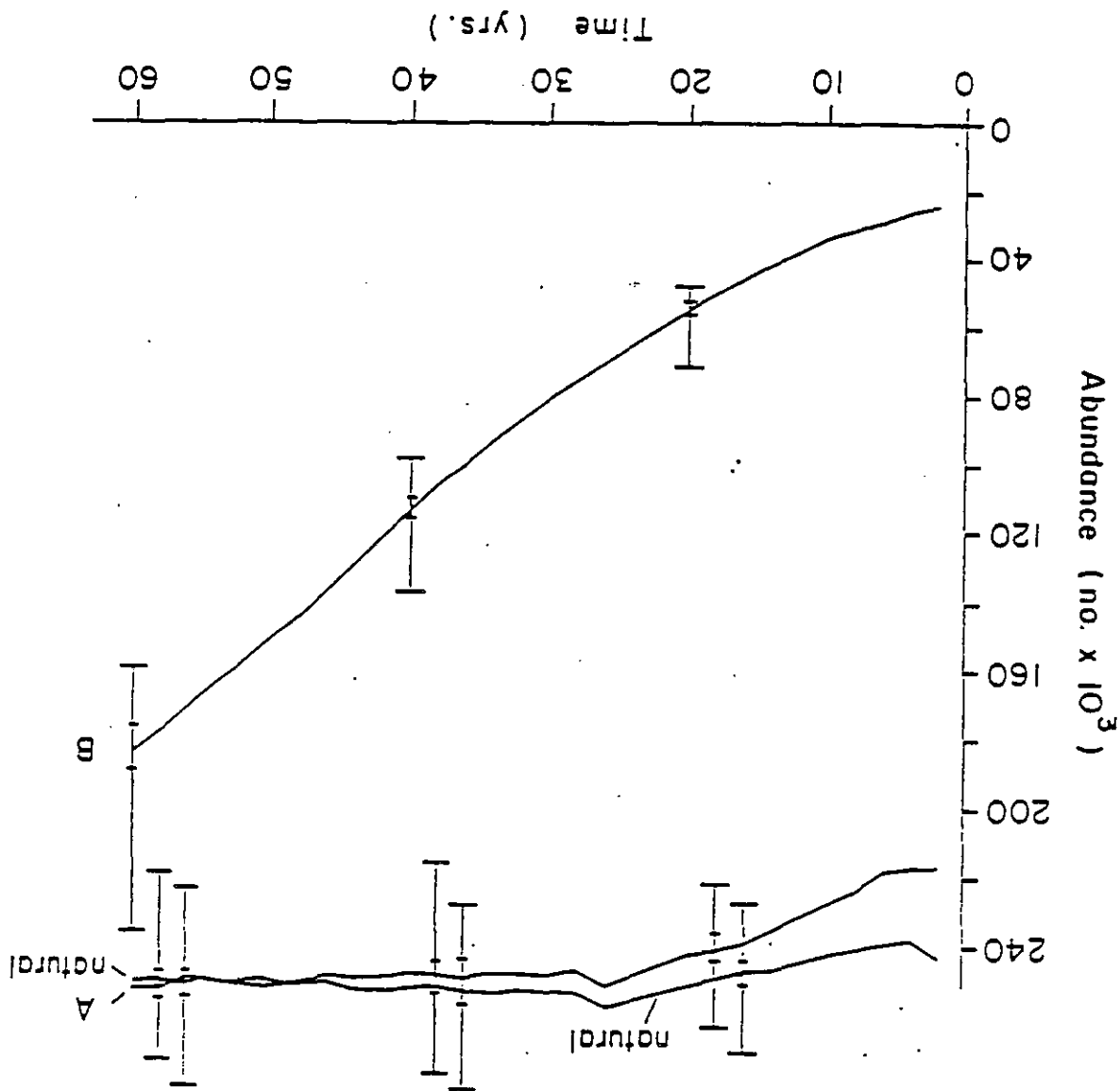


Figure 8. Expected changes in the total abundance (excluding age class 0) of murres over 60 years after a catastrophic decrease of population numbers (initial) and with weak density dependent regulation.

A. catastrophic decrease of initial population size by 5%

B. catastrophic decrease of initial population size by 20%

The run for the natural (unimpacted scenario) was continued for only 20 years. Ranges and 95% confidence intervals are presented for representative points only. The short vertical bars are 95% confidence intervals and wide vertical bars are ranges.

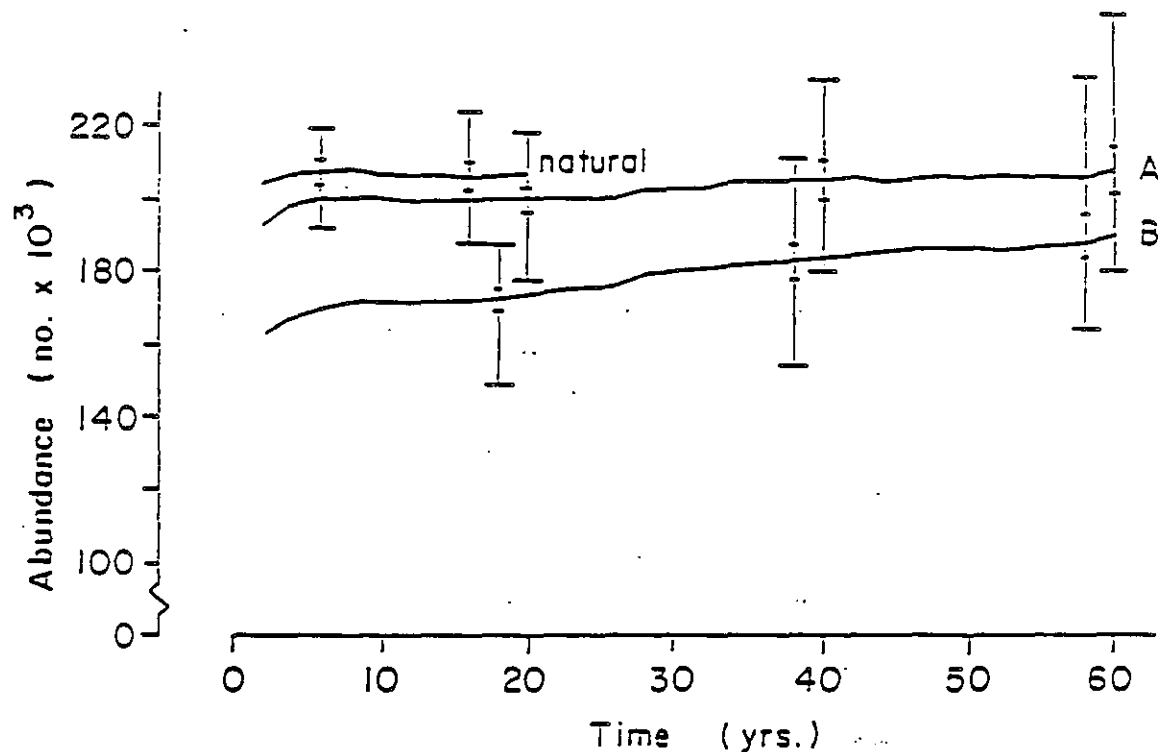
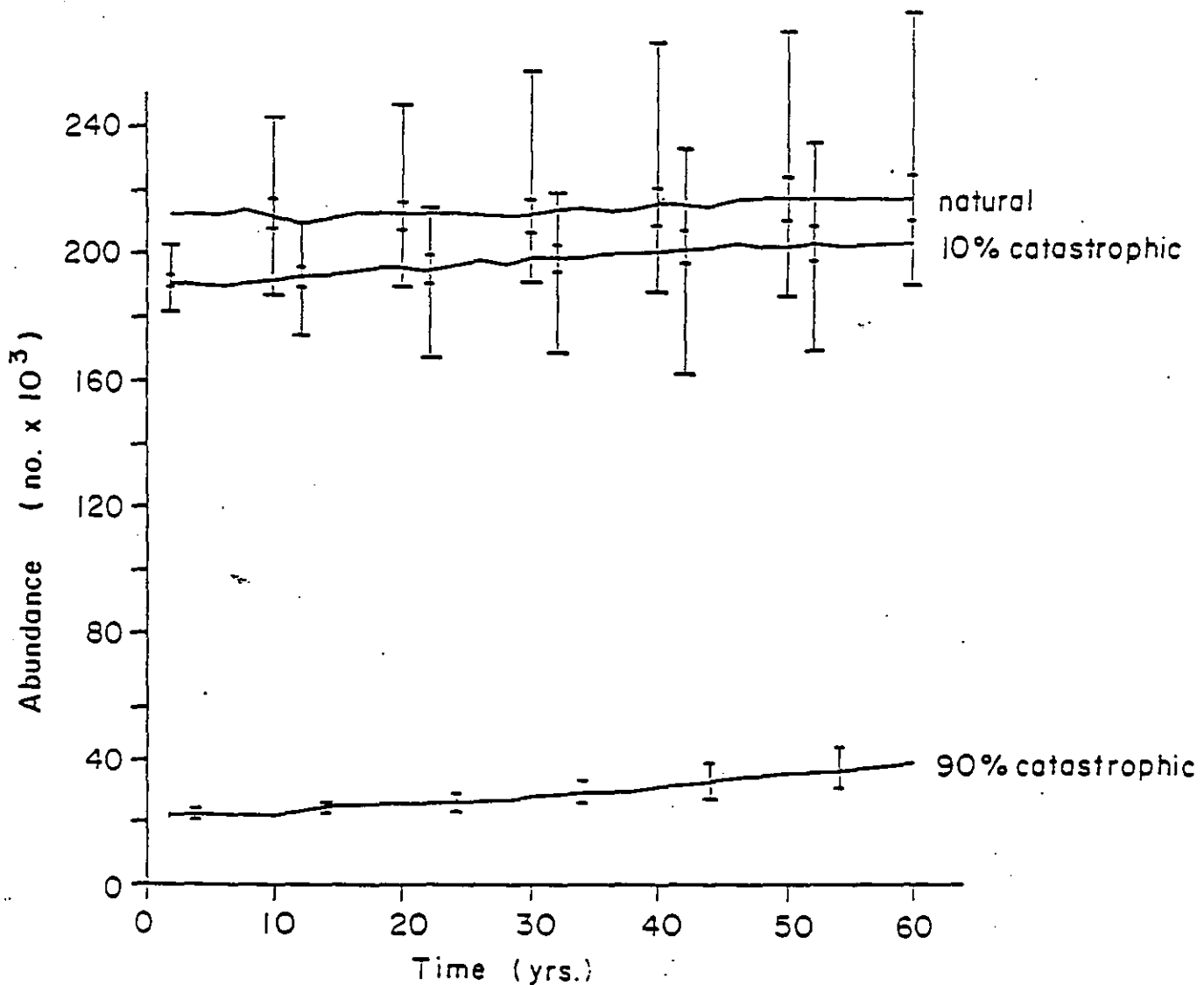


Figure 9. Expected changes in the total abundance (excluding age class C) of murres over 60 years after a catastrophic decrease of population numbers (initial) and with weak density dependent regulation.

A. catastrophic decrease of initial population size by 10%

B. catastrophic decrease of initial population size by 90%

Ranges and 95% confidence intervals are presented for representative points only. The short vertical bars are 95% confidence intervals and the wide vertical bars are ranges.



declines in population size over time, decreases of confidence intervals related to proportional relationships offset increases of confidence intervals caused by accumulated uncertainty.

2. Chronic Oil Spill Scenarios

Chronic exposure to low level oil pollution may potentially affect murre populations in many ways. A percentage of birds will die because of encounters with floating oil and consequential increases of energy loss and expenditure (see reviews by Brown, 1982; Clark, 1984). Declines of food availability related to oil impacts and the direct toxic effects of oil ingestion will further increase mortality (Brown, 1982; Clark, 1984). Egg production, hatching success, and chick growth may be decreased and the time of egg laying delayed. Delayed onset of egg laying may have serious consequences for Thick-billed Murres that breed in the short summers of the high Arctic (Birkhead and Nettleship, 1980).

It was hoped that ecotoxicological data could be used to estimate the effect of chronic exposure to oil pollutants on murre population parameters. Unfortunately, suitable data were not available. The effects of chronic oil pollution will depend on the concentration and type of pollutants, the timing of exposure relative to breeding state, constant versus fluctuating exposure, the effect on food supplies, etc. (Clark, 1984). Some possible pathways of effects through food sources are discussed in Section III. Ecotoxicological data presently available in the literature usually do not allow for reasonable extrapolations to nature and most often they examine effects on birds only distantly related to murres.

We examined the potential impact of chronic exposure to low level oil pollution by considering a range of possible impacts (Table 2). Biannual survivorship of each age classes was decreased by .02, .05, .07, and .10 with both weak and strong density-dependent regulation. These decreases in survivorship are equivalent to increases of adult mortality of 17%, 43%, 60%, 86% and 142% respectively. Changes in first year survivorship are relatively small (4 to 20%). One computer run with a 0.1 decrease in biannual fecundity and one run with a decrease in biannual survivorship of 0.2 were also examined.

The expected life of the Hibernia project is approximately 20 years (Mobil Oil E.I.S., 1985). Further oil finds may extend the period of oil exploration and development on the Grand Banks. The influence of chronic oil pollution was modelled for 20 years with both weak and strong density dependence and for 60 years with weak density dependence only. The results of the chronic impact scenarios for a period of 20 years are presented in Figures 1 and 2 and the results of the 60 year simulations are shown in Figure 3. The uncertainty of model predictions are expressed as 95% confidence intervals and ranges. On some figures, range bars are omitted for clarity because inclusion of all bars would make the graphs difficult to read. The probabilities of a murre population falling below various abundance levels after 20 and 60 years with the different scenarios are presented in Figures 4 and 5. For example, in Figure 4, the predicted probability that the unimpacted murre population with strong density-dependent regulation (A) will fall below 195,000 birds (see arrow) is approximately 0.15. The predicted probability that the same population, subjected to a biannual decrease of survivorship of .02 (B), will fall below

a size of 195,000 is 0.9, according to RAMAS. These plots indicate the range of model outcomes after 30 iterations based on the input parameters.

As expected, populations with weak density dependent regulation are less able to compensate for the effects of decreased survivorship than are populations with strong density-dependent regulation. The simulation results demonstrate the extreme sensitivity of murres to small changes in mortality. For example, even with strong density-dependent regulation, an increase of adult mortality of less than one-half (43% or a decrease of biannual survivorship of 0.05) results in a mean population decrease of 30% in only 20 years (Figure 1.B). An increase of adult mortality of less than two times (86% or a decrease of biannual survivorship of 0.1) results in a mean predicted decrease of population size of 50% in 20 years with strong density-dependent regulation (Figure 1.C). If biannual survivorship is reduced by 0.1 over 60 years with weak density dependent regulation, the mean predicted population size falls to 10% of equilibrium levels (Figure 3.C). If the abundance of murres fell to this level, there would be a serious danger of population extinction.

Birkhead (1977) examined the effects of the density of Common Murres on breeding ledges at a colony on Skomer Island, Wales. His results indicate that at low population densities, murres are less able to resist predation by gulls than at high densities. Birkhead (1977) and Hudson (1985) have suggested that this mechanism may accelerate any murre population declines at very low densities. There may also be a breakdown of the "information center" effect at murre colonies after severe population declines. Gaston and Nettleship (1981) have argued that murres at breeding colonies indirectly aid other murres in finding areas of high food availability. Mortality factors that increase in intensity with population declines ("depensatory mortality") were not included in the RAMAS models but may be of considerable importance.

3. Catastrophic Oil Spill Scenarios

It is extremely difficult to predict the number of birds that will potentially die in a catastrophic oil spill incident. Brown et al. (1973), Brown and Johnson (1980) and Brown (1982) emphasize that there is no obvious relationship between the size of an oil slick and the number of birds killed. For modelling purposes, considerations must be made of the path and size of oil slicks, the distribution and turnover on the water of birds of different ages and sexes, the probability of mortality of birds upon encountering a slick, behavioral responses of birds to an oil slick, changes in energy intake and expenditure with oil spillage, and associated changes in fecundity and chick and adult survivorship. (For a more in-depth discussion of these problems, see Ford et al., 1982 and Wiens et al., 1984).

Trudel (1983) has modelled murre mortalities with oil spillage at Hibernia. Unfortunately, many of the assumptions of the Trudel (1983) model are overly simplistic and unrealistic. Comparison of data from tanker oil spills near Nova Scotia and Newfoundland (Brown et al., 1973; Brown and Johnson, 1980; Brown, 1982) and distributional data for murres (from the PIROP database) reveal that the expected mortalities would be much greater than those predicted by the Trudel model. Ford et al. (1982) have developed an elaborate model to predict oil spill impacts on murre populations in the

Bering Sea, but the relevance of their results to the Hibernia area is unclear.

We examined a range of possible catastrophic scenarios (Table 2). Initial population densities were reduced by 5, 10, 20, and 90%. These declines each respectively correspond to mortalities of approximately 11,000, 22,000, 44,000 and 198,000 birds from one colony. Catastrophic population reductions of 90% are likely to occur only in the event of a major oil spill near a breeding colony (eg. Green Island, Witless Bay) or during the swimming migration. Two catastrophic scenarios in conjunction with decreases of 2 percentage points in biannual adult survivorship were also examined.

The results of the catastrophic scenarios are presented in Figures 6 to 9. As before, populations with strong density-dependent regulation (Figures 6 and 7) responded much more rapidly after a catastrophic reduction than did populations with weak density-dependent regulation (Figures 8 and 9). The time for the population to return after a 10% reduction to within 90% of the equilibrium densities was 16 and 10 years for populations with weak and strong density dependent regulation, respectively. After a 20% reduction of population numbers, return time to 90% of equilibrium densities was 46 years with weak density dependent regulation and 18 years with strong density-dependent regulation. The return time estimates with strong density-dependent regulation are similar to estimates made by McLaren (1985) and Hudson (1985) using deterministic Leslie Matrices and similar life table data. McLaren (1985) estimated a return time to 90% of equilibrium densities of 15 years for razorbills (a seabird closely related to murres) subjected to a 35% catastrophic reduction. Hudson (1985) predicted a return time of 12 years for murre colonies in eastern Canada after a 20% reduction of breeding adults. The slow response of murre populations to catastrophic decreases is a reflection of their low level of reproduction and their high adult survivorship in the unimpacted situation.

Populations of Thick-billed Murres are probably presently declining (Evans and Waterston, 1976; Gaston and Nettleship, 1981; Brown and Nettleship, 1984a; and Nettleship, 1985). Modelling of catastrophic reductions of population size in conjunction with increased chronic mortality (17%) resulted in accelerated decreases of population size (Figures 6C and D). Model populations in chronic decline did not increase after catastrophic reductions.

III. DIRECT AND INDIRECT EFFECTS AT THE ECOSYSTEM LEVEL

Many of the problems identified with the Hibernia E.I.S. are common to many other assessments. Ecosystems are complex systems that will always be partially specified. We will never know everything we would like to know for assessing and predicting impacts from developments. It is clear, however, given the tremendous numbers of potential pathways of effect from an introduced pollutant to a particular ecosystem component, and the massive number of potential network configurations, that some sort of modelling is necessary to reduce the complexity to a manageable level. Many recent approaches to environmental impact assessment include some form of modelling.

Much of the intensive ecological modelling of the last 25 years has involved large scale, quantitative, computer simulations of specific ecosystems. Often ecologists measure everything possible in the field in hope of having enough information to construct their models. In this type of modelling, emphasis is on fitting site-specific parameters and often little attention is given to the underlying structure of the ecosystem. The nature and number of links connecting pairs of components is largely ignored. Although the technology of model construction has greatly improved, there has not been a commensurate increase in ecological understanding. More recently, a form of structural analysis has been developed, called loop analysis. It permits the elucidation of underlying structure so that only those components and links crucial to system dynamics need be quantified. This methodology promises to be cost-effective in application to environmental impact and risk assessment problems, and to improve our current methods for predicting the role of both direct and indirect effects of pollutants in perturbed ecosystems.

A. Methods

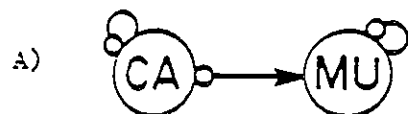
1. Loop Analysis

Loop analysis is a network technique that uses qualitative information to represent systems of interacting variables as signed digraphs. Once the network is constructed the effects of an environmental perturbation (termed a parameter input) on all variables can be predicted as qualitative changes in abundance levels. Patterns of correlation can also be predicted for each pair of directed changes related to a particular parameter input. In addition, community structure related to network properties and stability measures based upon feedback relationships can be generated from loop analysis. Like computer simulation models, loop models require three types of information: 1) variables, 2) interactions between pairs of variables and 3) identity and entry point of the driving function (systems analysis) or parameter input (loop analysis). Unlike systems analysis, loop analysis does not center upon the functional forms of the state equations but rather their partial derivatives evaluated at equilibrium and expressed as qualitative signs (Levins, 1973; Lane and Levins, 1977; Lane, 1985a,b,c and d).

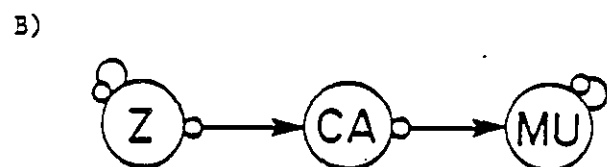
Three sample loop diagrams of portions of the Grand Banks pelagic ecosystem are illustrated in Figure 10. The large circles indicate variables and the line between two variables portrays the qualitative

Figure 10. Sample loop diagrams of some components of the Grand Banks ecosystem

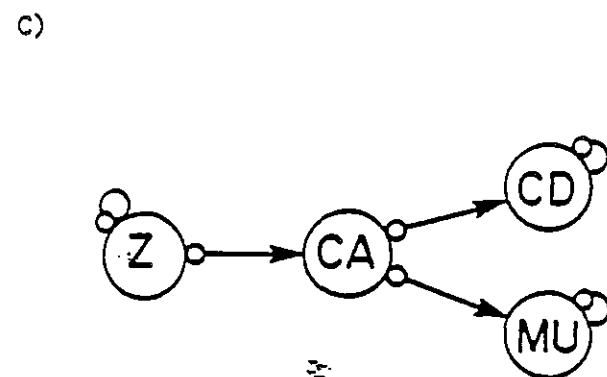
PREDICTION TABLES



Parameter Input	Effect	
	CA	MU
-CA	-	-
-MU	+	-



Parameter Input	Effect		
	Z	CA	MU
-Z	-	-	-
-CA	+	-	-
-MU	-	+	-



Parameter Input	Effect			
	Z	CA	MU	CD
-Z	-	-	-	-
-CA	+	-	-	-
-MU	-	+	-	+
-CD	-	+	+	-

sign(s) of their interaction. Lines between pairs of variables may have arrowheads or circleheads at each end. Arrowheads represent positive effects on the variables they touch and circleheads represent negative effects on the variables they touch. Negative self-damping terms are shown by a link coming from a variable and then touching itself with a circlehead. All models shown here assume negative parameter inputs of oil development and pollution.

In Figure 10.A, murras (MU) are feeding on capelin (CA). This is an example of a standard predator-prey relationship common in aquatic food webs. Both species are self-damped. The capelin are self-damped because their self-damping represents the lower portion of the food web (nutrients, algae and zooplankton) not included in the diagram and murras are self-damped because they have sources of mortality not illustrated in the model. To the right of the loop models, prediction tables are given to illustrate the effect of decreasing each of the ecosystem components. If capelin are decreased through oil pollution, then both capelin and murras decrease in population abundance levels. If only murras are decreased by oil directly, then capelin would increase in this simple model.

In Figure 10.B, zooplankton (Z) are added as a food of capelin and capelin are no longer self-damped. Out of nine possible predictions, seven will result in population decreases. Two oil pollution effects are positive. First, if capelin are impacted, zooplankton will increase since as prey organisms they will be released from predation; and second, if murras are decreased, capelin will increase because they will be released from murre predation.

In Figure 10.C, large fish including cod (CD) are added as predators to the capelin population. In this example, a negative parameter input to zooplankton decreases all components in the food web whereas a decrease of either large fish or murras at the top of the food web will increase their prey (capelin) and the other predator (MU or CA). If capelin are decreased then zooplankton increase whereas both capelin predators (MU and CD) decrease.

In Figure 10.A, there are two components, four links, three feedback loops and four possible pathways of effect. In Figure 10.B, there are three components, six links, four feedback loops and nine pathways. In Figure 10.C, with the addition of only one other variable, there are now nine links, six feedback loops and sixteen possible pathways.

From these simple examples, several conclusions can be made:

- 1) Prediction of what will happen to a particular ecosystem component when perturbed cannot be made without understanding how that component relates to the rest of the ecosystem. Cause and effect are intricately interrelated. Direct effects can be swamped by indirect ones in nature; thus, knowledge of direct effects is not enough.
- 2) As the number of variables increases (arithmetically) in the system under consideration, the number of potential pathways of effect increases much more quickly (exponentially). A modelling

tool such as loop analysis is needed to analyze this type of complexity.

3) The effects of a perturbation can be manifest in an ecosystem several steps or links away from the initial impact on a particular component. Thus, components not directly linked to each other can nevertheless exhibit profound influences on each other.

2. Model Assumptions

Application of loop analysis involves two assumptions: steady state and linear system dynamics. First, steady state refers to any state of a system where there is no ongoing change. Mathematically, this is defined as any point where the derivatives of the functions expressing the rates of change of all system variables are zero. Steady state for a given graph system may exist at only one point, several points, a continuum of points, at all points or no points. In stability analysis, it is assumed that the system being modelled is at steady state. The question of stability involves the system's behaviour around the steady state. Small changes in the system variables because of random fluctuations or other unknown causes are called perturbations. A perturbation could be visualized as picking the system up from where it is resting at steady state and placing it a short distance away. If after a perturbation in any direction, the system tends to move back to its steady state, it is said to be stable. If a perturbation can result in the system remaining where it has been moved to, or tending to move further from the steady state, the system is said to be unstable.

Second, because loop analysis is related to dynamic systems theory, interactions are based on linear functions which are represented by the signs of the slopes of such functions. Thus, it is assumed that variable change is proportional to the levels of other variables in the system. Any interactions which have non-linear aspects will be represented by a linear approximation. Such non-linear aspects include curved-line relationships (quadratic, exponential, logarithmic, reciprocal) with non-continuous slope or leveling-off functions, and relationships where the combined effect of two variables is different from their sum. The requirement for linear functions is not so strict for loop analysis since only the behavior of the system around steady state is relevant. In many cases, the linear approximations reflect the dynamics of the real system accurately enough that there is no difference in the results. If there are significant differences, they will be ignored by the analysis. Problems may arise, however, when interactions are involved whose functions change sign over some range of interest, usually in the neighborhood of steady state. If the researcher is careful to make sure that the model is based on functions which reflect system behavior around the current steady state, there will rarely be a problem with the analysis. The system, however, may not follow the same behavior away from the steady state or at a different steady state. To date, in using loop analysis with marine data sets, it has been possible to identify links that are potentially non-linear and to study their effects on the rest of the network. If a variable is known to be consistently non-linear, then a function of the variable may be constructed which is linear and the model can be recast using the transformed variable.

3. Results

Loop analysis can be used in two ways. First, intuitive loop models can be constructed and predictions calculated for various hypothetical perturbations to the environment. Second, loop models can be fitted against data sets that represent temporal changes in a set of ecosystem components as illustrated in Appendix D. For the Grand Banks ecosystem, the data were not available nor was there enough time to perform a full, data-based loop analysis. Instead, the first method was used and a variety of experts and literature references were consulted to gain an understanding of the basic interrelationships of the organisms and how they could be aggregated. This analysis is not meant to be exhaustive but rather to illustrate how pathways of effect from oil pollution could subsequently cause changes in the Grand Banks ecosystem. The analysis also illustrates how feedback relationships could be expected to operate in this ecosystem and how they would influence particular pathways of effect. Definitions of terms and calculation formulas are given in Appendix C.

The loop model for the pelagic zone of the Hibernia area in winter is given in Figure 11. The key to the variables is given at the bottom of Table 3. The model has fifteen components and 38 interaction links. The bottom portion of the food web involving nutrients, microzooplankton, algae, and some miscellaneous non-copepod zooplankton groups has been omitted. This lower part of the food web has been extensively modelled for a variety of sites in the Western Atlantic by Lane (1985 a,b,c and d) using the second method (fitting against data sets which include temporal changes) and a composite model for this portion of the food web is described in Appendix D. Given the species lists and some abundance data for Hibernia it is probable that the lower portion of the network would be similar but this has not been tested (Figure D.1). Several hundred species have been individually located as to their functional role in the lower portion of the food web. For the present study, the upper portion of the food web was emphasized to aid understanding on how murre and their response to oil development and pollution would affect the rest of the pelagic ecosystem and conversely how impacts on the rest of the food web by oil contamination would affect murre. On the Grand Banks in winter, approximately 80% of the murre would be Thick-billed Murre and the rest Common Murre (Brown, pers. comm.; Wendt and Cooch, 1984).

In winter, capelin (Mallotus villosus) are the main prey of murre (Uria spp.) (Tuck, 1961). Capelin are also preyed upon by a variety of large fish species including Atlantic cod (Gadus morhua). Murre can dive to 80 meters and, as such, can exploit the capelin stocks effectively (Piatt et al., 1984). Other small fish including the sand lance (Ammodytes spp.) and herring (Clupea harengus) consume arrowworms or chaetognaths (several species). Small fish are preyed upon by marine mammals. At this time of year, these would be mostly harp seals (Phoca groenlandica). A group of surface-feeding seabirds (DF) which would consist mostly of kittiwakes (Rissa tridactyla), and fulmars (Fulmarus glacialis) are represented as consuming euphausiids (Thysanoessa spp.). In addition, there are two groups of adult copepods (Z1 and Z3) each with their group of invertebrate predators (PD1 and PD3) respectively. Z1 includes the copepod genus Calanus spp. and this group is preyed upon by some gelatinous macro-invertebrates

Figure 11: Loop model of the upper portion of the pelagic ecosystem on the Grand Banks in the winter. Key to variables and predictions for negative impacts related to oil pollution are given in Table 3.

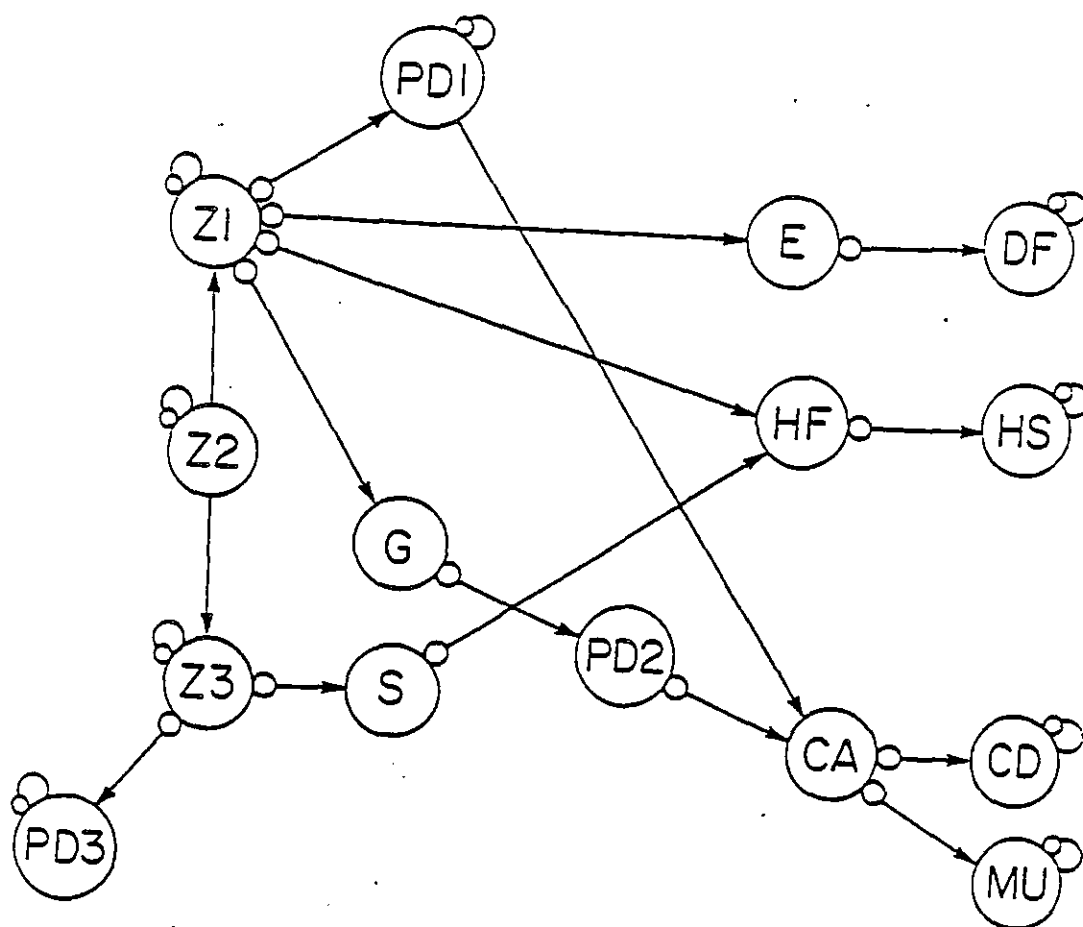


TABLE 3: Predictions of qualitative changes in all of the ecosystem components assuming a negative environmental (parameter) input to each component listed in the left hand column

Parameter	Effect ---->															
Input	Z1	Z2	Z3	PD3	HS	CD	PD1	DF	MU	E	G	PD2	CA	S	HF	
-Z1	-	0	-	-	-	-	-	-	-	-	-	-	-	+	-	
-Z2	?	-	-	-	-	?	?	?	?	?	?	?	?	?	-	
-Z3	+	0	-	-	-	+	+	+	+	+	+	+	+	-	+	
-PD3	-	0	+	-	+	-	-	-	-	-	-	-	-	+	+	
-HS	-	0	+	+	-	-	-	-	-	-	-	-	-	-	+	
-CD	-	0	-	-	-	-	-	-	+	-	+	-	+	+	-	
-PD1	+	0	+	+	+	?	-	+	?	+	?	+	?	-	+	
-DF	-	0	-	-	-	-	-	-	-	+	-	-	-	+	-	
-MU	-	0	-	-	-	+	-	-	-	-	+	-	+	+	-	
-E	+	0	+	+	+	+	+	-	+	-	+	+	+	-	+	
-G	+	0	+	+	+	-	+	+	-	+	-	-	-	-	+	
-PD2	-	0	-	-	-	-	-	-	-	-	+	-	-	+	-	
-CA	+	0	+	+	+	-	+	+	-	+	-	+	-	-	+	
-S	+	0	+	+	-	+	+	+	+	+	+	+	+	-	-	
-HF	+	0	-	-	-	+	+	+	+	+	+	+	+	+	-	

Key to variables:

Z1	=	adult copepods, group 1 (including <u>Calanus</u> spp.)
Z2	=	immature copepods
Z3	=	adult copepods, group 2
PD1	=	miscellaneous predators of adult copepods (Z1)
PD2	=	predators of gelatinous zooplankton (G) including hyperiid amphipods
PD3	=	miscellaneous predators of adult copepods (Z3) including decapods, coelenterate larvae, hydromedusae
HS	=	marine mammals especially harp seals
CD	=	large fishes including Atlantic Cod
DF	=	other non-murre seabirds that feed on the surface
MU	=	murres
E	=	euphausiids and possibly mysids
G	=	gelatinous zooplankton including ctenophores
CA	=	capelin
S	=	chaetognaths
HF	=	small fishes including sand lance and herring

which are in turn preyed upon by PD2. This latter group includes several species of hyperiid amphipods. Fish larvae and squid are assumed to be absent or in very low numbers at this time of year.

Experts could argue at length over the ecological detail that this model embodies and especially about the feeding relationships that are represented. Our major argument concerning the poor treatment of causality and indirect effects in the Hibernia E.I.S., however, does not pivot on the existence of an exact and accurate loop model of this ecosystem. Further development of the model and rigorous testing with the existing data sets,

however, could certainly delineate the ecosystem structure more conclusively than that represented in Figure 11.

Our main point is to demonstrate by example the degree of complexity involved with understanding basic causal relationships and predicting impacts at the ecosystem level and how poorly the proponent has performed this task. Tools, such as loop analysis, could greatly enhance current methods of environmental assessment and impact prediction.

In the Hibernia loop diagram, (Figure 11), there are 52 feedback loops and 282 valid pathways of effect among all pairs of variables. The maximum number of paths is 52 for paths of length 5. There are also 51 paths of length 6. There are only 29 valid paths of length 2 and 15 paths of length 1. The direct effects are represented by paths of length 1; paths of length greater than 1 represent indirect effects. Thus, the sheer number of longer paths argues for the need to elucidate indirect pathways of effect and not rely on short, direct pathways for understanding the effects of perturbation. In addition, direct pathways are often reversed in sign (+ to - or - to +) when ecosystem level feedback is operative.

In the preceding section on risk analysis, predictions were made concerning population levels of murres given that they were subjected to catastrophic oil spills or chronic oil pollution. Several of the hypothetical scenarios produced results that showed a substantial impact on the murre populations on the Grand Banks. Uncertainty measures were also included to emphasize that no one can predict with absolute surety the exact number of murres in the future with or without oil pollution.

Aided by reasonable models, however, it is possible to place bounds on our estimates and to substantially improve upon unaided intuition. Since murres are such an important ecosystem component and since they are so long lived, a substantial decrease in murres caused by either catastrophic oil spills or chronic oil pollution could lead to a decrease in most ecosystem components in the loop model except large fish (CD), gelatinous zooplankton (G), capelin (CA) and chaetognaths (S) as shown in Table 3 (directed change predictions for the loop model of the Grand Banks). In addition, although murres could be directly impacted by oil, they could also suffer if an oil-related perturbation entered the ecosystem through adult copepods (Z1), invertebrate predators (PD2), marine mammals (HS), seabirds (DF), gelatinous zooplankton (G), and capelin (CA). A negative impact on certain other ecosystem components (Z3, CD, E, S and HF) could improve murre population levels. Two ecosystem components (Z2 and PD1) are predicted to have ambiguous effects on murres because both positive and negative pathways operate between Z2-MU and PD1-MU. These ambiguities could be resolved with semi-quantitative analysis (Lane and Levins, 1977).

IV. SUMMARY

In this study, two methodologies were employed to address some of the deficiencies in the assessment methodology for the Hibernia E.I.S. in which ecological prediction, uncertainty, and risk as well as ecosystem-level causality were poorly treated by the proponents. We have presented examples of two modelling methods to illustrate how the E.I.S. deficiencies could be rectified. The first methodology used was a population level risk analysis involving a stochastic form of the Leslie Matrix. This approach was developed by Dr. L. Ginzberg and is implemented in a software package named RAMAS. It predicts changes in abundance using population parameters represented by a probability distribution, rather than a single value as is used traditionally with Leslie Matrix methods. RAMAS also calculates the probability of a population falling below a threshold abundance during a given time period.

The second methodology is termed loop analysis and was applied to the pelagic zone of the Grand Banks ecosystem during winter. This is a period when oil extraction and shipment become more difficult and environmental risk increases. For both modelling methods, murre, an important seabird, were used as the ecological component of interest. Time and resources did not permit an exhaustive approach with either modelling methodology. The results illustrate, however, what it is possible to achieve with more rigorous approaches to environmental impact assessment.

Scenarios of unimpacted populations were contrasted with chronic and catastrophic impact scenarios using RAMAS. Model parameters for the natural scenario were chosen after consultation with local experts and the literature and were adjusted slightly to establish population stationarity.

In the chronic impact scenarios, small changes of life-table parameters resulted in relatively large decreases in population size. Chronic increases of annual mortality rates by 17 to 86% over 20 years resulted in mean population declines of 11 to 50% with strong density-dependent regulation. An increase of adult mortality by 86% over 60 years could seriously threaten population survival. The ability of a population to compensate for chronic increases of mortality is particularly sensitive to the level of density dependent regulation attributed to it. Even with strong density dependent regulation, however, relatively large population decreases were predicted.

The model demonstrated that the response of murre populations to catastrophic events was slow as a result of their low egg production and delayed reproduction. RAMAS calculated return times, after population reductions to 90% of equilibrium densities, of 46 years with weak density-dependent regulation and 18 years with strong density-dependent regulation. Increased knowledge of recent population trends, density-dependent relationships and the impact of oil on murre at the individual level is required to improve future population-level risk predictions.

A loop model was constructed and analyzed for the upper portion of the food web for the pelagic zone of the Grand Banks during winter. It consists of 15 components and 38 interaction links with 52 feedback loops of various lengths. Loop analysis results demonstrated that negative inputs to murre related to environmental deterioration with oil development would lead to

mostly negative changes in the rest of the food web and most certainly impacts would not operate on murre in isolation. Negative changes to other ecosystem components were predicted to affect murre both negatively and positively.

Of the 282 valid pathways of effect operating in this food web, long paths are numerically dominant. Direct (short) pathways are also dominated by long indirect ones in a qualitative sense in that the sign of the direct effect can be reversed when the total food web is considered. Thus, ecological effects of a pollutant on the ecosystem level can be counterintuitive in comparison with known (one link) physiological or population-level relationships.

The loop model graphically illustrates the degree of complexity involved in making realistic impact predictions at the ecosystem level and illustrates how inadequate the proponents' approach is because of their focus on population-level phenomena, lack of quantification and their poor treatment of ecosystem-level effects and ecological risks.

The E.I.S. can also be strongly criticized for containing almost no mention of risk in the ecological portions of the document and not even a feeble attempt to consider risk. The inadequate EIA methodology directly leads to a number of problems in conclusions and their interpretation. The proponents side step the risk problem by stating emphatically and repeatedly that there are no impacts or only small, negligible ones. If one writes statements of complete certainty assuming the Grand Banks is a "determined" ecosystem that is completely understood then there is no place for uncertainty and consequently no need to calculate risk. This overall approach is entirely unrealistic and should not be allowed to continue in environmental impact statements prepared for the offshore.

There has also been a marked shift elsewhere in environmental studies from using the terminology, "assessing the impact" to "assessing the risk". The change in terminology reflects the growing realization that we cannot make environmental predictions in a deterministic way with absolute surety. To believe otherwise is to delude ourselves. Previously, the reliance on deterministic models for ecosystems masked the importance and prevalence of ecological uncertainty. The inherent stochasticity of environments is confounded by human interventions which are often cumulative, multiplicative, indirect and counterintuitive. This argues for the need to develop objective risk analysis tools for ecological systems so that uncertainty can be quantified and evaluated rigorously. If this is not done, risk-benefit decisions involving the environment will continue to be made with a poor scientific basis and often with the strongest-interest group winning. the risk-benefit trade off has to be made explicit and this can only be done with a firm quantitative basis.

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